

THURSDAY, SEPTEMBER 17, 1896.

LYDEKKER'S GEOGRAPHICAL HISTORY OF MAMMALS.

A Geographical History of Mammals. By R. Lydekker, B.A., F.R.S., V.P.G.S., &c. Cambridge Geographical Series. Pp. xii + 400. (Cambridge: University Press, 1896.)

THERE can be no doubt about Mr. Lydekker's qualifications to enter on the field of geographical distribution. The author of the excellent treatise on mammals in the "Royal Natural History," after serving an apprenticeship on the Indian Geological Survey, has arranged and catalogued the splendid series of remains of extinct mammals in the British Museum. Alone of European palæontologists, he has likewise visited the rich collections recently amassed in the museums of Buenos Ayres and La Plata. He has thus the advantage, not possessed by any previous writer on the subject, of a more intimate acquaintance with the past history of mammals than perhaps any other living naturalist has been able to accumulate, and on the present occasion has made good use of it.

In the main outlines of his scheme of geographical regions, as propounded in the introductory chapter of the present work, Mr. Lydekker follows generally the well-known arrangement of Sclater and Wallace; but, as we shall presently show, deviates from their views in several important particulars. As to the correctness of the primary division of the earth's surface into "Notogæa," "Neogæa," and "Arctogæa," all authorities, we believe, whose opinions are worthy of consideration, are now nearly in accord. This arrangement was first proposed by Mr. Sclater in 1858, in one of the "Manchester Science Lectures," though other titles were then given to the two last-named divisions. In 1890, Dr. Blanford adopted the same primary areas with slight alterations in the names. The "anonymous writer" in *Natural Science*, who, in 1893, assigned the names "Notogæa," "Neogæa," and "Arctogæa" to these three divisions, we take to have been Mr. Sclater himself, or some one inspired by him. At any rate, these are the terms adopted by Mr. W. L. Sclater in his articles on the "Geography of Mammals," lately published in the *Geographical Journal*; and we agree with Mr. Lydekker in regarding them as the simplest and best-selected names yet proposed. But, having proceeded thus far, we have only arrived at the front of our difficulties.

"Arctogæa" embracing the whole land-surface of the world except Australia (*Notogæa*), and South and Central America (*Neogæa*), requires subdivision. Messrs. Sclater and Wallace have proposed to effect this in the most simple and natural way, by making four "regions" out of "Arctogæa"—namely, the Ethiopian, Oriental, Palæarctic and Nearctic, and thus to recognise six primary zoological regions. They admit, of course, that these six regions are not of exactly equal value. But in such a matter, as in all other classifications, convenience should be consulted to a certain extent, and the "six regions" are very convenient, being readily defined and easily recognisable, and are much more in accordance with facts

than any other regions that have yet been suggested. Mr. Wallace has set all this fully forth in an address to the Cambridge Philosophical Society, which was published in this journal in April 1894.¹ We regret to observe that Mr. Lydekker scoffs at this excellent and well-reasoned article, and speaks of it as an attempt to "bolster up" a lost cause. Yet he continually refers to Mr. Wallace's writings throughout his work, and acknowledges his eminence as an authority on geographical distribution.

Mr. Lydekker summarises his objections to the "six regions" of Sclater and Wallace as follows:—

"It has the serious drawback that it gives no greater rank to Australasia and South America than to the other divisions; whilst the remarkable difference between the faunas of Africa and Madagascar is overlooked. Further, the northern parts of America are widely separated from those of Europe and Asia, to which they are faunistically allied."

Mr. Lydekker proposes the following modified scheme to meet the defects thus specified.

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| I. Notogæic realm | (1) Australian region. |
| | (2) Polynesian region. |
| | (3) Hawaiian region. |
| | (4) Austro-Malayan region. |
| II. Neogæic realm | Neotropical region. |
| III. Arctogæic realm | (1) Malagasy region. |
| | (2) Ethiopian region. |
| | (3) Oriental region. |
| | (4) Holarctic region. |
| | (5) Sonoran region. |

We venture to assert that these suggested modifications only lead Mr. Lydekker into further difficulties.

In the first place, realm is only region "writ short," and is hardly sufficiently distinct to be used in a different sense. Why not say simply "Notogæa," "Neogæa," and "Arctogæa"—three orthographically constructed compounds, of which the meaning is patent to all acquainted with the subject? Again, the so-called "Polynesian region" is a mere appendage of the Australian region. As Mr. Lydekker well says, it is "characterised by the general absence of non-flying mammals." But the birds—the next important group—are mostly Australian in character, though a few genera are autochthonous. The Moas (*Dinornithide*) which, until lately, played such an important rôle in it, are certainly as nearly related to the Cassowaries as to any other group. Lories (a specially characteristic Australian type) are scattered over it. In fact, Polynesia can only be properly placed as one of the sub-regions of the Australian region.

Much the same may be said of Mr. Lydekker's "Hawaiian region." The only mammal of the Sandwich Islands is a bat. The land-birds are certainly very peculiar, and mostly restricted to the group. It is, as yet, not ascertained to what outside forms they are most nearly related; it is quite certain, however, they have nothing to do with America. But to rank the Hawaiian Islands as constituting a division equal in value to the Ethiopian region is simply impossible. The alleged inequality of Sclater and Wallace's six regions is a trifle compared with this feat of Mr. Lydekker. The best place for the so-called "Hawaiian region," so far as our present knowledge goes, is within the boundaries of the Australian.

¹ See "What are Zoological Regions?" (*NATURE*, vol. xlix. p. 612).

Again, the "Austro-Malayan region" of Mr. Lydekker is merely a border-land between the Australian and Oriental regions, and has no sort of claim to the rank here assigned to it. It has few, if any, indigenous types of mammals, and cannot for a moment be put on a par, as it is in Mr. Lydekker's scheme, with the Ethiopian region, which has numerous families, both of mammals and birds, restricted to its area. Mr. Lydekker's "Austro-Malayan region," except Celebes—which is certainly a difficult subject—may be safely annexed to the Australian region. Celebes has been also referred there by Mr. Wallace, but on the whole we opine that it would be better placed as a distinct sub-region of the Oriental region.

We now come to Mr. Lydekker's "Arctogæic realm," or "Arctogæa," as we prefer to call it. This is divided by Mr. Lydekker into five regions, as shown above. As regards the separation of Madagascar and its islands from the Ethiopian region, under the name of the "Malagasy region," there is much to be said in its favour, and we do not deny that our author has some good grounds to go upon. It is obvious that the mammal fauna of Madagascar, as well shown by Mr. Lydekker (see p. 215 of his work), is one of the most extraordinary on the world's surface—not only for what it has, but still more, perhaps, for what it has not. We can, therefore, offer no serious objection to Dr. Blanford's proposal (accepted by Mr. Lydekker) to raise the rank of Madagascar from that of a sub-region (as it has been treated by Messrs. Sclater and Wallace) to that of a full-blown region.

Mr. Lydekker's Ethiopian and Oriental regions remain much the same as Mr. Wallace's; but as regards the next two—the Holarctic and the Sonoran, there is a wide difference. Mr. Lydekker, misled by Dr. Merriam and other American writers, who take a narrow view of the subject, proposes to annex the northern part of America to the northern part of the Old World, calling it altogether Holarctic; while the more southern part of North America, down to the boundaries of the Neotropical region, is denominated "Sonoran." To assent to this proceeding, however, would only involve us in further difficulties. Most of the "Sonoran" mammals penetrate far into the north, outside its supposed limits. On examining the list of the Sonoran types (p. 379) and that of the "Western Division of the Holarctic region" (p. 344), we shall find them meagre indeed, and quite insufficient to support a distinction between two regions. The polar area may, in fact, be safely regarded as border-land between the Palearctic and Nearctic regions. It must be recollected that Northern America was, in comparatively recent days, covered by the polar ice-sheet even much more extensive than that of Northern Europe. This destroyed nearly all animal life, and drove most of the remainder into Mr. Lydekker's "Sonoran region." On the disappearance of the ice-sheet the northern land was naturally repopulated from the adjacent part of Asia across Behring's Straits, as well as from the Sonoran region. Hence, no doubt, came such characteristic Palearctic forms as the sheep, the bison, the mountain-goat, and the stag into North America. Of these, however, all but the mountain-goat have penetrated into the Sonoran region, and we have some doubts whether

Haploceros is not likewise to be met with within its supposed boundaries.

Another serious objection to the "Holarctic region" is that, as regards birds at least, by adopting it we shall mix up some of the most characteristic forms of the New World in the same primary division as those of the Old World. Take, for example, the humming-birds—a most eminently typical group of the New World. Humming-birds range all over Canada in the summer, and on the west of the continent pass up to Alaska. Following Mr. Lydekker's scheme, we should have to place the Trochilidae in the "Holarctic list." The same would be the case with the Mniotiltine warblers (*Mniotiltidae*), the greenlets (*Vireonidae*), the hang-nests (*Icteridae*), and other forms which are absolutely restricted to America, and utterly foreign to the Old World (*Paleargian*) avifauna.

On the whole, therefore, we cannot doubt that Mr. Lydekker would have been more prudent to stick to the old-fashioned "six regions." Even had he not quite agreed to them, he might have sheltered himself under Mr. Wallace's authority, and safely followed his leadership.

In his intimate acquaintance with fossil mammals, Mr. Lydekker had, as we have already stated, a great advantage over his fellow-workers in the same field, and one of which he has not failed to make good use in some of his arguments. This branch of the subject is certainly much more completely discussed in the "Geographical History of Mammals" than in any other work of the same character, and we are duly grateful to the author for the many novel facts he has thus set before us. At the same time it should be recollected that, while we are pretty well acquainted with the present mammal-fauna of the earth and the facts of its distribution, we know comparatively little about the past. The "imperfection of the geological record" should be always in our minds when arguments are used taken from the little that is yet known of the ranges of extinct mammals, our notions of which may in many cases come to be seriously modified by discoveries yet to be made.

On the whole, however, we must allow that Mr. Lydekker's volume forms a valuable contribution to the "Cambridge Geographical Series," and that the general editor has done wisely in securing such a well-written essay on this branch of his subject from a paleontological point of view. Although we notice a few typographical errors, the volume is well printed, and excellently illustrated by numerous process-blocks introduced into the text, and by a chart of the zoological regions. Altogether it contains a large mass of information reduced into a small compass, and will meet, we are sure, with generous appreciation from all students of distribution.

THE RATIONAL STUDY OF PLANT-DISTRIBUTION.

Lehrbuch der Ökologischen Pflanzengeographie eine einföhrung in die kenntniss der Pflanzenvereine. Von Dr. Eugen Warming. Deutsche Ausgabe von Dr. E. Knoblauch. (Berlin: Gebrüder Borntraeger, 1896.)

AN account of the principles underlying the facts of the geographical distribution of plants has long been a desideratum. Although various persons have written on the subject, they have not, for the most part,

approached it from the point of view which, thanks largely to the often decried "laboratory system," we are enabled to do at the present time. In fact, until botanists had given up restricting their attention to species, and to the grosser external characters of plants, it was not possible for them to apprehend how intimately the welfare, and consequently the distribution, of the organism and of the species is bound up with minute and often apparently trivial details of structure. It is true that the general characters of what we may term the *Habitus* of groups of plants had been more or less clearly defined. Humboldt and Grisebach had already distinguished numerous dominant types, and had indicated the general nature of their relationships.

But what we want to find out is the causal *nexus* which exists between the plant, and the locality or conditions in which it lives. It is this, the biological, aspect of the question which is the important one for to-day. We are deeply conscious that life is a struggle between conflicting organisms more or less adapted to the conditions of life to which they are exposed. We know, too, that in this struggle, no factor is without its due weight in determining the final result. But we cannot hope to unravel the tangle of reasons which may account for the presence of this type here and its absence there, nor can we appreciate the nice adjustment between the individual constituents which compose the type, until we are in a position to investigate the inter-relations existing between the adaptation and the environment to which it responds. Before this could be, it was first necessary to obtain an insight, not only into the minute details of anatomy, but also into their connection with the functions discharged by the organism as a whole. Only then can we appreciate the true meaning of the peculiarities presented by members of such characteristic floras as alpine, epiphytes, mangrove swamps, and the like.

It is not that the problems of distribution have hitherto attracted but little interest—far from it—but that before they could be successfully grappled with, a laboratory training formed an indispensable preliminary. But it is only a preliminary. It is all very well to study collections of plants, whether in the form of pickled material, or herbarium specimens, or even as living beings in hot-houses. It is only by travelling, and seeing the things as they actually grow under natural conditions, that one is in a position to estimate the importance of this or that structure, and its relation to the welfare or existence of the species. It may not be necessary to travel far in order to make some progress in this study. Our own country affords abundant opportunity to those who know how to use their eyes; still, there can be no question but that it is in tropical regions that the *purposefulness* of structural modifications most forcibly obtrudes itself on the mind of the observer.

The questions involved are most fascinating, and they are most intricate. Hence it is the more important that we should address our inquiries in an orderly manner if we are to successfully analyse and classify the numerous factors concerned. To indicate how this may be done is one of the objects of Prof. Warming's book, and he may fairly claim to have largely succeeded in his efforts.

He discusses, in the first place, the general effect of physical conditions on plant-life; and his remarks are

always interesting, even where we do not quite agree with the conclusions to which he arrives. He then gives a short classification of the different characteristic groups of plants, which he assembles in four different divisions—the Hydrophytes, Xerophytes, Halophytes and Mesophytes, the last including what we may term normal vegetation. The key-note to his treatment of these four divisions is given in the ideal which he keeps before him, that of ascertaining the manner in which each type and each species places itself in harmony with its surroundings by means of morphological, anatomical, and physiological differentiation and adaptation. The book is essentially one of classification of these adaptations, and of the varied environments inhabited by plants, and it is one which ought to be read not only by botanists, but by all who care for the general questions concerning the distribution of living forms in water and on land.

J. B. F.

OUR BOOK SHELF.

Rivers and Canals. The Flow, Control, and Improvement of Rivers, and the Design, Construction and Development of Canals, both for Navigation and Irrigation; with Statistics of the Traffic on Inland Waterways. By Leveson Francis Vernon-Harcourt, M.A. In 2 vols. Vol. i., Rivers; vol. ii., Canals. 651 pp. and index; with 13 plates of illustrations. (Oxford: Clarendon Press, 1896.)

THE first edition of Mr. Vernon Harcourt's book on rivers and canals was published in 1882, and has been regarded as one of the standard books on the subjects of which it treats. The present edition is not merely a revise of the former one, but has been almost entirely rewritten, and the subjects rearranged and brought up to date. The wide experience which the author has had, from being frequently called upon professionally to investigate and report on matters relating to rivers and harbours, and the active interest he has taken in the various navigation congresses which have been held in this and other countries during the last few years, fully entitle him to write with authority on the theory of river engineering, and the principles to be observed in carrying out works of improvement. The theoretical part of the book is supported by descriptions and illustrations of the chief works which have been carried out for the control and improvement of rivers, and the construction of canals. The book is written in a style that is thoroughly readable, and is not encumbered with detailed facts and information which, although of great value to an experienced engineer, are not required by a student or reader who wishes to become acquainted with general principles. On the whole, as would naturally be expected, the views expressed by the author are sound, and such as have received general acceptance by the most experienced engineers of this and other countries. There are, however, some matters dealt with on which engineering "doctors differ," and in these cases Mr. Vernon Harcourt would, perhaps, have added to the value of his book if he had given a little more credit to the views of other engineers who have devoted their attention to the same subject. The illustrations are very clear and effective, and add considerably in elucidating the descriptions in the text. In fact, both the author and the publisher deserve the thanks of the engineering profession for bringing up-to-date a work bearing on the management of our harbours and rivers, on the efficiency of which the prosperity of the navigation and commercial interests of this country so largely depend.

Elementary Practical Chemistry and Qualitative Analysis.

By Frank Clowes, D.Sc. Lond., and J. Bernard Coleman, A.R.C.Sc. Pp. xvi + 224. (London: J. and A. Churchill, 1896.)

THIS book, which is founded on Prof. Clowes' larger "Practical Chemistry and Qualitative Analysis" is intended for the use of general students and of technical students in schools and colleges who are desirous of acquiring a general elementary knowledge of chemistry, and who propose to acquire this knowledge in the only true way, viz. by themselves performing experiments in a laboratory. For such students the book furnishes an admirable guide. The first eighty pages contain excellent instructions as to the preparation and use of apparatus, the methods of carrying out ordinary chemical operations, and the modes of demonstrating the properties of common gases and liquids. The remainder of the book is occupied with a course of qualitative analysis, which treats first, at considerable length, of the reactions for metals and for acid-radicles, and then of the actual analysis of simple and complex substances. There is, further, an appendix of useful tables and a good index.

The hand of the experienced and careful teacher is manifest throughout. The importance attached to cleanliness, neatness, and system in the rules given for working; the directions for the verification of the statements made, and for the keeping of the student's notebook; the precautions indicated as necessary for success in certain experiments, the careful attention to detail, and the emphasis given just where it is needed, show that the authors have knowledge not only of chemistry, but also of the "general and technical student," who, if he will observe the instructions, and work fairly through the book, cannot fail to acquire a real knowledge of his subject.

For boys and girls at school, we ourselves should recommend a course on somewhat different lines, starting, for instance, with air rather than with oxygen, following generally a historical sequence, taking the various chemical operations not *en bloc*, but as required in the course, and relegating qualitative analysis to a comparatively subordinate place. But taking things as they are, and accepting as a fact the existing requirements of various public examining bodies, this little work should prove widely useful as a carefully-arranged, clear, and accurate text-book.

Entomological Notes for the Young Collector. By William A. Morley. Pp. viii + 129. (London: Elliot Stock, 1896.)

THIS is a little book of the most popular kind, written with the intention of rendering the collecting of butterflies and moths easy to the youngest of beginners. It is illustrated by eight pages of figures representing apparatus, setting, &c., and the text is divided into twelve chapters, corresponding to the months of the year, each including a lesson on apparatus, collecting, rearing, &c., and a list of some of the principal *Lepidoptera* which appear in each month. The book may be useful to those for whom it is intended; and we congratulate the author on his good judgment in advising his readers to learn the Latin names, and to forget the English. Here and there a little revision would be useful; thus Entomology is defined as "that branch of natural history which bears special reference to four-winged insects known as butterflies and moths" (no other insects being even mentioned in the book); Cambridgeshire is the only locality given for *Papilio machaon* and *Vanessa antiopa*; *Lycæna artaxerxes* is said to be "generally distributed in England"; of *L. corydon*, we read "On chalk cliffs, common"; and moths which come to sugar are said, as a rule, not to come to light.

W. F. K.

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LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. No notice is taken of anonymous communications.]

The Utility of Specific Characters.

IT is dangerous for a mere mortal to take part in the strife of the gods, or for "gyrating" mathematicians to join battle with biologists. But as the enthusiasm of Prof. Weldon for his subject has so largely perturbed my normal gyrations, that I have devoted many months to the statistical theory of evolution, perhaps I may be permitted a word or two on the subject of the present controversy.

To demonstrate that natural selection, whether secular or periodic, is actually taking place in any species, and to measure its amount, is in the present state of our knowledge one of the grandest pieces of work that could be done. It would achieve for the Darwinian theory what Hertz achieved for the Maxwellian theory of light. At present no one has gone further in the direction of this demonstration than Prof. Weldon, and I am inclined to think with Mr. Thiselton-Dyer—and I take it in opposition to Prof. Lankester—that such a demonstration can only be achieved by the statistical method. If, however, we are to obtain a really solid result of that method, then the mathematical theory, and the logic used, must both be beyond suspicion. Now in any demonstration of the existence of natural selection two points must be borne in mind:

(a) A selective death-rate must be actually demonstrated. This is a problem for fine statistical theory.

(b) The correlation between organ and death-rate must be shown in itself to be not fortuitous. The character must have been selected because it is useful. This I take to be Prof. Lankester's point.

I propose to say a few words as to both (a) and (b).

It appears to me that both Prof. Lankester and Mr. Thiselton-Dyer allow that a selective death-rate has been established in the report on *Carcinus menas* of 1894-5. This view I take to be entirely erroneous, and I so expressed myself to Prof. Weldon and several members of the Committee before the Report was published. What Prof. Weldon demonstrates is this—that if crabs chanced to grow in a particular manner, then there would be a relation between death-rate and the size of a certain frontal ratio. What is quite certain is that at the time the Report was published, nobody knew how crabs grew; and I very much doubt whether Prof. Weldon, after his laborious two years' study of the growth of crabs, would now uphold the hypotheses he then adopted, e.g.:

- (i.) That size could be taken as a safe measure of age.
- (ii.) That young crabs of the same frontal ratio do not "scatter" as they grow older.
- (iii.) That the amount of growth of crabs of any given frontal ratio is entirely independent of that ratio.

Yet if these—to me very improbable—hypotheses be not accepted, the supposed demonstration of a selective death-rate in *Carcinus menas* falls completely to the ground. The very hypothetical character of the conclusions of the Report of 1894-5, appears by his letter of August 26 to be now very fully recognised by Prof. Weldon himself. I am not, however, sure that it has been generally recognised. When the law of growth of crabs has been accurately ascertained, then I am convinced that it will require much more complex analysis than that of the Report to ascertain whether a selective death-rate does or does not exist. I should not have said so much on this first point did I not believe that next to blindly rejecting natural selection, the most dangerous course open to biologists is to accept a proof of its existence which is sure one day to be demonstrated as fallacious by one of the many opponents of Darwinism.

On the second point, surely Prof. Lankester is entirely in the right? It is not sufficient to show that there is a correlation between a certain frontal ratio and death-rate in order to assert that the frontal ratio is a cause of death-rate. Very probably it may be, but the demonstration is not logically complete, or at any rate a definition of cause has been adopted which does not appear

¹ The term is due to Prof. Lankester, who thus described us—I think it was to Mr. Thiselton-Dyer—in the early days of the Teaching University movement.

of much utility to science. If the height of the barometer be correlated with the frequency of explosions in mines, it would not appear utile to describe the barometer as a "cause" of the explosion. Or, to take another case, which is purely hypothetical, but which will, I think, illustrate Prof. Ray Lankester's point. There is, we will suppose, a purely random distribution of supernumerary teats in cows. But in my particular herd the two best milkers possess supernumerary teats (although there is no correlation between such teats and good milkers in general). I keep the calves of these two cows because they are good milkers, and by reason of this selection supernumerary teats become more and more common in my herd. At last I begin to preserve calves with supernumerary teats, really because this is a test of their descent from the good milkers, practically because I find them in themselves good milkers. Now, because I am a careful breeder, my cows may get a reputation at sales all over the country, and a correlation between supernumerary teats and good milkers may come to be generally recognised. This happens not because supernumerary teats are a cause of good milkers, but owing in the original instance to a random association of this variation with a utile variation. Thus, a primarily random association with a favourable variation may by the principle of heredity quite easily lead to a correlation which it would be of no profit to consider causal. If two characters be not correlated, and one be favourable to survival, then any selection of the favourable character, which hits a group of individuals having more than the average of the second character—and this may easily arise if we breed from comparatively few individuals—will by the principle of heredity lead to a fortuitous correlation. I do not assert that this is the case in the frontal ratio of crabs, but it seems to me that a link is really missing in the chain of demonstration. All causality is of course correlation, but the converse, which Prof. Weldon seems to hold and Prof. Lankester to controvert, is surely a dangerous doctrine?

KARL PEARSON.

September 10.

Specific Characters among the Mutillidæ.

THE discussion in your columns as to the utility of specific characters leads me to offer a few remarks on the Mutillidæ, an interesting family of Hymenoptera. In the arid region of the United States, this family is very numerously represented, and its members may be seen running about in warm weather, especially frequenting sandy places, roads and pathways. It is not at first apparent why the species should be so numerous, living under what seem to be identical or almost identical conditions; in 1893 (*Trans. Amer. Ent. Soc.*, xx. 343), I wrote: "It is difficult to account for the origin of so many species under conditions which can hardly at any time have been very diverse." But the region in question is inhabited by very many species of bees, the modifications of which have relation to a varied flora, as I have illustrated by particular instances elsewhere (*Proc. Ac. Nat. Sci. Phila.*, 1896, pp. 33-41). The various Mutillidæ are parasitic in the nests of these bees, and consequently do not live under identical conditions; we have a varied flora with its varied insect-visitors, and these with varied parasites, the whole series of phenomena intimately connected, though at first sight it would seem impossible to see any connection between the flowers and the mutillids, however indirect.

It must be a long time before the actual host-relations of all the mutillids are known, but I have a species now under observation, which may serve as an example. The bee *Diadasia diminuta* lives in colonies, burrowing perpendicular tunnels in the beaten pathway, which are produced somewhat above the level of the ground by means of fragile cylinders of sandy particles, designed to keep the tunnels from being filled with sand. The little *Sphærophthalma heterochroa* is the parasite of this bee, and the females may be seen in numbers running about between the burrows, now and then looking into them or entering. At once we see the utility of one of the specific characters of *S. heterochroa*—its small size. The larger species could not enter the small burrows of the bee.

The females of *S. heterochroa* are splendid little insects, ornamented with scarlet, black, and whitish. Like the females of all Mutillidæ, they are wingless. The much more active winged males, which are not so elegantly ornamented as the females, may be seen bustling about, looking for the latter. In

the Mutillidæ, the females are very varied in colour, markings and structure; while the males are much more uniform. Thus, Cameron says ("Biol. Cent. Amer. Hymenoptera," p. 259): "This general resemblance of the males not only makes their specific determination a work of difficulty, but it adds greatly to the task of assigning them to their respective partners of the opposite sex." If the bright and varied colours and markings were due to activity or a "katabolic tendency," it is in the winged males that they ought to be found; not, as is actually the case, in the wingless females. But on the principle of utility there may be an explanation. The males have to look for their respective females, and I believe the ornamentation of the latter assists their recognition.

There is a whole series of Mutillidæ which are very plainly coloured, from tawny through various shades to black, never with any scarlet, or conspicuous markings. These (*Photopsis* and *Brachycistis*) are all nocturnal, without any exception, and come to lights in the evening. But the systematists who have described many of these insects, were totally unaware of this circumstance until I pointed it out recently!

The moral of all this is, that to understand the real meaning of specific characters we must study the species in nature. We are hardly more likely to understand natural phenomena from the examination of dead animals alone, than a Hottentot would be to understand the apparatus of telegraphy. And eventually, I believe even the pure systematist will have to base his work on biological observation. It has been fondly hoped all along that absolute criteria of specific distinction would be found in the insects themselves, without reference to their habits; and the searchers for such "hall-marks," driven from point to point, have at length taken refuge in the male genitalia. But only a few days ago I received the following in a letter from M. Ernest André, the distinguished French student of Hymenoptera, and particularly Mutillidæ.

"Comme je l'ai dit, je crois qu'on attache aujourd'hui une trop grande importance aux caractères tirés de l'appareil génital mâle. Ces organes sont très variables, difficiles à apprécier, et ne concordent pas toujours avec les autres caractères morphologiques." T. D. A. COCKERELL.

N.M. Biological Station, Mesilla, New Mexico,

U.S.A., August 25.

The Khmer of Kamboja.

IN NATURE, June 11, p. 135, I see a short notice of the work being done in Australia by Prof. R. Semon, of Jena, and that he classes the "Khmer and Chams of Kamboja" as "primitive Dravido-Australians." I hope that some of your anthropological experts will, as soon as possible, correct this serious mistake.

Mr. A. H. Keane, in NATURE, January 6, 1881, p. 222, calls these people "Caucasian"; but I presume they are now (1896) known to be what Captain C. J. F. S. Forbes classed them, *i.e.* Mon-Anam, in his "Languages of Further India."

J. R. Logan, in his "Ethnology of the Indo-Pacific Islands," published at Singapur and Pinang, in the *Journal* of the Indian Archipelago (1847-63), rightly classes them as Mon-Anam, giving their linguistic peculiarities and alliances, pronouns, &c., and numerals up to ten, those from one to five being identical with our Kol, Sontal, Munda, Ho (the most western relatives of the Mon-Anam alliance), and quite different to the Dravidian, numerals.

The now civilised Kambojans admit that the "Khmer dom" are the older and purer stock, whence they are descended, and that they were *hill savages*, which carries out what we know so far of these early pre-Burmo-Tibetan races from the Asam side.

Both in physique and languages, the Dravido-Austral aboriginal of India (south of Himalaya) and the Mon-Anam group are very distinct; the former are seen purest in the Andamani and Negrito, in whom there is an entire absence of Tibetan elements.

But the Mon-Anam (which includes the Khmer) were formed, as a race, by the mixture, or fusion, of (Sifan) Tibetans with the Dravidians, lying south of Himalaya from Nipal to East Asam. At one time this "Mon-Anam" race appears to have covered all Northern and Eastern Bengal, the whole of Asam,

and the Ultra-Indian peninsula from Asam to Singapore, and even to have extended to the Nicobars, Sumatra, Borneo, and beyond. In India, on the west and south-west, the Tibetan element dies out, and gives place (gradually) to the purer Dravidian; and here we have the darker Kol alliance, Munda, Santali, &c., say, roughly, two-thirds Dravidian and one-third Tibetan. Further east we have the relics of the Mon in the Bodo, Koch, Mech, of the Delta, and the Garo-Kasia of the eastern hills, the latter more Tibetanised by later influx of Tibetans *vis à vis* Bhutan, in physique and language.

Then we have a vast gap filled in by later "Tibeto-Burman" races, and come to the "Mon" of Pegu, who show the influence of the Tibeto-Burman inroads, though retaining still considerable "Kasia" affinities. The Kambojans, again, are another fragment of the "Mon," having specific affinities with Manipuri and Naga. Anamese, again, is distinguished by its strong Manipuri, Barak, and Kol affinities, and showing Chinese influence, through contiguity becoming more monosyllabic.

Logan, who was an expert in these matters, tells us that "the Mon-Anam pronouns and numerals are partly Tibetan and partly Dravidian—chiefly the latter—but most of the substantial roots are similar to Tibetan, and the forms more archaic than the current Tibeto-Burman." "The difference between Dravido-Australian and the Mon-Anam formation is so great, that it may be safely connected with the equally striking difference of race, and ascribed to a long-continued and total ethnic separation during its earlier history. The Simang and Andamani are the purest remnants of a pre-Himalaic race in Ultra-India, and it is probable that similar Dravido-Australian tribes [lived there] before the Mon-Anam entered the region."

But one of the best proofs that the "Khmer" are not Dravido-Australian is, that the Australian races all have numerals on the binary basis 1 and 2. Three is 2+1 or 1+2. Four is 2+2. Five is 2+2+1. This was formerly the basis of the Dravidian system, long before the Mon-Anam (Khmer, &c.) arose as a race.

The Australians, in fact, whose languages are known to be "more nearly allied to the South Indian than to any other in the world," left India (probably *vis à vis* Malaya) when the Dravidian numerals were in their earliest binary stage, and before the quinary and denary stage was developed (anywhere).

As Logan says, "the quinary and denary systems, with the Dravidian mode of forming 8 and 9, indicate affinities belonging to much later periods. The civilisation which originated them, was unknown to Dravido-Australian at the time when the early Asonian migrations took place."

That the Mon-Anam race, much later on, passed south and east from India, and extended even over the Archipelago and Pacific, is becoming yearly more obvious, not only through study of physique and customs, but the number of roots, pronouns, and vocables having a *Himalaic* basis. But from their site of origin, in Bengal, as a (locally varied) admixture of Tibetan and Dravidian, the racial development and tribal drift may have been exceedingly slow, like the drift we see now among the much later Tibeto-Burman, Lushai Kuki, &c., taking probably many centuries in crossing from the Delta, across the Barak, Manipuri, and Naga Ranges, and *vis à vis* Asam. That they at one time covered the entire Ultra-Indian peninsula, is obvious from the position of the fragments of the race, due to the intrusion of the Tibeto-Burmans, but that they extended to the east of the Upper Irawadi is doubtful.

The Cham, Charay, Stieng, Xong, Samré, and Kuy are the less known, more barbarous, and purer branches of the Mon-Anam living in the Mekong highlands. S. E. PEAL.

Sibsagar, Asam, August 11.

Dr. Siemens' Smokeless Open Grate.

DR. C. WILLIAM SIEMENS described fully in NATURE (November 11, 1880, vol. xxiii. p. 25) a smokeless open gas and coke or gas and anthracite grate for living rooms, costing about one halfpenny per hour in fuel. Will any users of this grate, who may have an extended experience of it, relate their views, and state any lessons its use may have taught them?

During an extensive hunt for a smokeless open cheap grate, Dr. Siemens' grate is the most satisfactory I have yet found.

FRED. WM. FOSTER.

Neckinger Mills, Bermondsey, September 9.

NO. 1403, VOL. 54]

THE LIVERPOOL MEETING OF THE BRITISH ASSOCIATION.

IV.

LIVERPOOL, September 14.

WE are now on the eve of the meeting. St. George's Hall opens to-day as the British Association reception-room; and although the attendance on the first day will be chiefly Liverpool people securing their seats for the evening addresses in the Philharmonic Hall, still a few visitors from other parts of the country and from abroad have already arrived, and many more are expected to-morrow. We are told that more associates' and ladies' tickets have now been taken than at any previous meeting on the corresponding day. These local ticket-holders, along with the members from elsewhere who have intimated their intention of being present, amounted on Saturday to about 2500, so there is every prospect of a large gathering.

The last meeting, for the present, of the Executive Committee was held on Friday, and the local secretaries then reported upon the final arrangements for the meeting. The Town Hall will probably prove too small for the number of members who will attend the first soirée, so the Lord Mayor (the Earl of Derby) has obtained permission from the Committee of the Exchange News-room, across the Exchange "flags," to utilise that magnificent hall, in addition to the Town Hall, for his entertainment. A covered way will connect the front of the Exchange News-room with one of the windows of the Town Hall.

At the second soirée, to be held in the Walker Art Gallery, Free Library, and Museum, the accommodation is ample, so provision is being made for short lectures, lantern demonstrations, and various exhibitions in the galleries and rooms.

Nothing has been changed in the other Sectional arrangements and fixtures already announced, but some additional information has come in regard to the probable work of Sections F, I and K.

In the Section of Economics and Statistics, the proceedings on Thursday will be opened at 11 a.m. by the Presidential Address, which will deal with economic teaching and political action. The treatment of this subject derives much additional importance from the almost unique position in the political world occupied by the President (the Right Hon. L. Courtney). In due consideration that the meeting takes place this year in a large and busy commercial centre like Liverpool, several sittings will be devoted to the consideration of matters of directly practical, as well as theoretic interest. On one day (Friday), three papers will be given by Messrs. Cannan, W. H. Smith, and Blunden on various points relating to the incidence of local rates and the municipal control of finance. On another day (Monday), future dealings in produce will form the subject of discussion. Papers dealing with these will be contributed by Mr. H. R. Rathbone, Mr. Charles Stewart, and Mr. E. Helm, of Manchester. On Tuesday, currency questions will occupy the attention of the Section, when it is hoped that a paper will be read in defence of the gold standard by Mr. William Fowler. One or two papers from the bimetallic side are promised. Other papers will deal with the effects of trade amalgamations, systems of economic education, standard of value and money, commercial crises, cotton prices, remedies for agricultural distress, metric system, and other important subjects. It is hoped that there will be included among these a consideration of some aspects of charitable and philanthropic trading, by Mr. C. S. Loch.

In the "Section of Physiology, including Psychophysics and Experimental Pathology," in addition to the Presidential Address and the discussions already

announced, some of the more important communications will be as follows:—"Further researches on the phonograph," and "Method of transmitting wave-forms from phonograms to paper," by Prof. McKendrick; "Method of distinguishing between organic and inorganic compounds of iron in the tissues," by Prof. Macallum (Toronto); "The glycoside constitution of proteid matter," by Dr. Pavy; "Photometry and Purkinje's phenomenon," by Prof. Haycroft; "Bacteria and daily food," by Dr. Kanthack; "The physiological effects of peptone when injected into the circulation," by Prof. Thompson; "The influence of glycerine on the growth of bacteria," by Dr. Markton Copeman; and other papers by Dr. Dupuy, Prof. Gotch, Dr. Mann, Prof. Boyce, &c.

In the Botanical Section, in addition to what we have already announced, there will be two discussions on subjects which have for some time past been prominently before the botanical world. On Friday, Mr. Francis Darwin will introduce a discussion on the movements of water in plants; others will be followed by a demonstration by Prof. Vines of a new method for the experimental investigation of some phenomena connected with the transpiration current. On Tuesday the second discussion will be opened by Prof. Farmer. This will treat of problems connected with the vegetable cell, but it is hoped that with the co-operation of zoologists the subject will be discussed from a general biological point of view.

An important contribution by Miss Sargent will be made after the discussion, in which some recent researches on nuclear division will be described.

On Monday afternoon, as already announced, a lecture will be delivered by Mr. Thiselton-Dyer, on the geographical distribution of plants. The subject will be treated in such a way as to appeal not only to specialists, but to the wider circle of those who possess a general interest in botanical science.

The papers to be communicated to this Section may be summarised as follows. Prof. Magnus (of Berlin), Mr. Ellis, and Mr. Vaughan Jennings have promised communications on the life-history of various Fungi. Dr. Mann is expected to make a communication on work carried out by Miss Huit on the physiology of the tentacles of *Drosera*; and from Dr. Morris (of Kew), there will be a contribution on some remarkable pathological effects produced by a West Indian weed, *Leucaena glauca*. On Monday, Prof. Chodat (of Geneva) will give an account of conclusions arrived at as the result of his important researches on Algæ, relating to such problems as the origin of sexuality, and the relationship of Algæ to the higher plants.

Prof. Bower and Mr. Lang (of Glasgow) will deal with investigations on Vascular Cryptogams, and from the latter author may be expected an exceedingly important contribution which has a direct bearing on the question of alternation of generations treated of in the President's opening address. Palæobotanical papers are to be read by the President and Mr. Seward on Palæozoic and Mesozoic plants. It is hoped that Prof. Trail (of Aberdeen) may give an account of his extensive researches on the variation in flower structure of the *Polygonaceæ*; and Mr. Scott-Elliott will probably read a paper on the effect of habitat on the habits of plants. Results of anatomical and physiological interest will be communicated by Mr. Keeble and Mr. Gwynne-Vaughan.

Although the hope that Nansen might have attended this meeting will not be realised, we shall have Sir Martin Conway fresh from Spitzbergen, and an account will be given by Mr. Montefiore Bryce of the experiences in Franz Josef Land of the Jackson-Harmsworth Expedition.

Amongst recent additions to the list of foreigners and Americans expected are:—Prof. Fokkar (Gröningen), Prof. Johannes Walthers (Jena), Dr. Montelius (Stockholm), Prof. Keeler (Pennsylvania), Dr. Stolpe (Stockholm), Prof. Zacharias (Hamburg), Dr. Herrschen (Upsala), Dr.

Johann Hjort (Christiania), M. Theodore Nica (Bucharest), Prof. Bohoslav Brauner (Prague), Prof. Nasini (Padua), Prof. Yves Delage (Paris), Prof. J. Berg (Stockholm), Prof. R. Chodat (Geneva), Prof. Kohlrausch (Berlin), M. J. Violle (Paris). Other distinguished guests who will attend were mentioned in a former article.

Before this appears in print the presidential and other opening addresses will have been given, and the character of the meeting will have declared itself; but as far as present indications afford a clue, we may expect an unusually large gathering, and a number of important discussions in the Sections. How far these forecasts and expectations have been realised will appear in our final article.

W. A. HERDMAN.

INAUGURAL ADDRESS BY SIR JOSEPH LISTER, BART., D.C.L., LL.D., P.R.S., PRESIDENT.

MY Lord Mayor, my Lords, Ladies, and Gentlemen, I have first to express my deep sense of gratitude for the great honour conferred upon me by my election to the high office which I occupy to-day. It came upon me as a great surprise. The engrossing claims of surgery have prevented me for many years from attending the meetings of the Association, which excludes from her Sections medicine in all its branches. This severance of the art of healing from the work of the Association was right and indeed inevitable. Not that medicine has little in common with science. The surgeon never performs an operation without the aid of anatomy and physiology; and in what is often the most difficult part of his duty, the selection of the right course to follow, he, like the physician, is guided by pathology, the science of the nature of disease, which, though very difficult from the complexity of its subject matter, has made during the last half-century astonishing progress; so that the practice of medicine in every department is becoming more and more based on science as distinguished from empiricism. I propose on the present occasion to bring before you some illustrations of the interdependence of science and the healing art; and the first that I will take is perhaps the most astonishing of all results of purely physical inquiry—the discovery of the Röntgen rays, so called after the man who first clearly revealed them to the world. Mysterious as they still are, there is one of their properties which we can all appreciate—their power of passing through substances opaque to ordinary light. There seems to be no relation whatever between transparency in the common sense of the term and penetrability to these emanations. The glasses of a pair of spectacles may arrest them while their wooden and leathern case allows them to pass almost unchecked. Yet they produce, whether directly or indirectly, the same effects as light upon a photographic plate. As a general rule the denser any object is the greater obstacle does it oppose to the rays. Hence, as bone is denser than flesh, if the hand or other part of the body is placed above the sensitive film enclosed in a case of wood or other light material at a suitable distance from the source of the rays, while they pass with the utmost facility through the uncovered parts of the lid of the box and powerfully affect the plate beneath, they are arrested to a large extent by the bones, so that the plate is little acted upon in the parts opposite to them, while the portions corresponding to the muscles and other soft parts are influenced in an intermediate degree. Thus a picture is obtained in which the bones stand out in sharp relief among the flesh, and anything abnormal in their shape or position is clearly displayed.

I need hardly point out what important aid this must give to the surgeon. As an instance, I may mention a case which occurred in the practice of Mr. Howard Marsh. He was called to see a severe injury of the elbow, in which the swelling was so great as to make it impossible for him by ordinary means of examination to decide whether he had to deal with a fracture or a dislocation. If it were the latter, a cure would be effected by the exercise of violence which would be not only useless but most injurious if a bone was broken. By the aid of the Röntgen rays a photograph was taken in which the bone of the upper arm was clearly seen displaced forwards on those of the forearm. The diagnosis being thus established, Mr. Marsh proceeded to reduce the dislocation; and his success was proved by another photograph which showed the bones in their natural relative position.

The common metals, such as lead, iron, and copper, being

still denser than the osseous structures, these rays can show a bullet embedded in a bone or a needle lodged about a joint. At the last conversazione of the Royal Society a picture produced by the new photography displayed with perfect distinctness through the bony framework of the chest a halfpenny low down in a boy's gullet. It had been there for six months, causing uneasiness at the pit of the stomach during swallowing; but whether the coin really remained impacted, or if so, what was its position, was entirely uncertain till the Röntgen rays revealed it. Dr. Macintyre of Glasgow, who was the photographer, informs me that when the presence of the halfpenny had been thus demonstrated, the surgeon in charge of the case made an attempt to extract it, and although this was not successful in its immediate object, it had the effect of dislodging the coin; for a subsequent photograph by Dr. Macintyre not only showed that it had disappeared from the gullet, but also, thanks to the wonderful penetrating power which the rays had acquired in his hands, proved that it had not lodged further down in the alimentary passage. The boy has since completely recovered.

The Röntgen rays cause certain chemical compounds to fluoresce, and emit a faint light plainly visible in the dark; and if they are made to fall upon a translucent screen impregnated with such a salt, it becomes beautifully illuminated. If a part of the human body is interposed between the screen and the source of the rays, the bones and other structures are thrown in shadow upon it, and thus a diagnosis can be made without the delay involved in taking a photograph. It was in fact in this way that Dr. Macintyre first detected the coin in the boy's gullet. Mr. Herbert Jackson, of King's College, London, early distinguished himself in this branch of the subject. There is no reason to suppose that the limits of the capabilities of the rays in this way have yet been reached. By virtue of the greater density of the heart than the adjacent lungs with their contained air, the form and dimensions of that organ in the living body may be displayed on the fluorescent screen, and even its movements have been lately seen by several different observers.

Such important applications of the new rays to medical practice have strongly attracted the interest of the public to them, and I venture to think that they have even served to stimulate the investigations of physicists. The eminent Professor of Physics in the University of College of this city (Prof. Lodge) was one of the first to make such practical applications, and I was able to show to the Royal Society at a very early period a photograph, which he had the kindness to send me, of a bullet embedded in the hand. His interest in the medical aspect of the subject remains unabated, and at the same time he has been one of the most distinguished investigators of its purely physical side.

There is another way in which the Röntgen rays connect themselves with physiology, and may possibly influence medicine. It is found that if the skin is long exposed to their action it becomes very much irritated, affected with a sort of aggravated sun-burning. This suggests the idea that the transmission of the rays through the human body may be not altogether a matter of indifference to internal organs, but may, by long-continued action, produce, according to the condition of the part concerned, injurious irritation or salutary stimulation.

This is the jubilee of *Anæsthesia* in surgery. That priceless blessing to mankind came from America. It had, indeed, been foreshadowed in the first year of this century by Sir Humphry Davy, who, having found a toothache from which he was suffering relieved as he inhaled laughing gas (nitrous oxide), threw out the suggestion that it might perhaps be used for preventing pain in surgical operations. But it was not till, on September 30, 1846, Dr. W. T. G. Morton, of Boston, after a series of experiments upon himself and the lower animals, extracted a tooth painlessly from a patient whom he had caused to inhale the vapour of sulphuric ether, that the idea was fully realised. He soon afterwards publicly exhibited his method at the Massachusetts General Hospital, and after that event the great discovery spread rapidly over the civilised world. I witnessed the first operation in England under ether. It was performed by Robert Liston in University College Hospital, and it was a complete success. Soon afterwards I saw the same great surgeon amputate the thigh as painlessly, with less complicated anæsthetic apparatus, by aid of another agent, chloroform, which was being powerfully advocated as a substitute for ether by Dr. (afterwards Sir James Y.) Simpson, who also had the great merit of showing that confinements could be conducted

painlessly, yet safely, under its influence. These two agents still hold the field as general anæsthetics for protracted operations, although the gas originally suggested by Davy, in consequence of its rapid action and other advantages, has taken their place in short operations, such as tooth extraction. In the birthplace of anæsthesia ether has always maintained its ground; but in Europe it was to a large extent displaced by chloroform till recently, when many have returned to ether, under the idea that, though less convenient, it is safer. For my own part, I believe that chloroform, if carefully administered on right principles, is, on the average, the safer agent of the two.

The discovery of anæsthesia inaugurated a new era in surgery. Not only was the pain of operations abolished, but the serious and sometimes mortal shock which they occasioned to the system was averted, while the patient was saved the terrible ordeal of preparing to endure them. At the same time the field of surgery became widely extended, since many procedures in themselves desirable, but before impossible from the protracted agony they would occasion, became matters of routine practice. Nor have I by any means exhausted the list of the benefits conferred by this discovery.

Anæsthesia in surgery has been from first to last a gift of science. Nitrous oxide, sulphuric ether, and chloroform are all artificial products of chemistry, their employment as anæsthetics was the result of scientific investigation, and their administration, far from being, like the giving of a dose of medicine, a matter of rule of thumb, imperatively demands the vigilant exercise of physiological and pathological knowledge.

While rendering such signal service to surgery, anæsthetics have thrown light upon biology generally. It has been found that they exert their soporific influence not only vertebrata, but upon animals so remote in structure from man as bees and other insects. Even the functions of vegetables are suspended by their agency. They thus afford strong confirmation of the great generalisation that living matter is of the same essential nature wherever it is met with on this planet, whether in the animal or vegetable kingdom. Anæsthetics have also, in ways to which I need not here refer, powerfully promoted the progress of physiology and pathology.

My next illustration may be taken from the work of Pasteur on fermentation. The prevailing opinion regarding this class of phenomena when they first engaged his attention was that they were occasioned primarily by the oxygen of the air acting upon unstable animal or vegetable products, which, breaking up under its influence, communicated disturbance to other organic materials in their vicinity, and thus led to their decomposition. Cagniard-Latour had indeed shown several years before that yeast consists essentially of the cells of a microscopic fungus which grows as the sweetwort ferments; and he had attributed the breaking up of the sugar into alcohol and carbonic acid to the growth of the micro-organism. In Germany Schwann, who independently discovered the yeast plant, had published very striking experiments in support of analogous ideas regarding the putrefaction of meat. Such views had also found other advocates, but they had become utterly discredited, largely through the great authority of Liebig, who bitterly opposed them.

Pasteur, having been appointed as a young man Dean of the Faculty of Sciences in the University of Lille, a town where the products of alcoholic fermentation were staple articles of manufacture, determined to study that process thoroughly; and as a result he became firmly convinced of the correctness of Cagniard-Latour's views regarding it. In the case of other fermentations, however, nothing fairly comparable to the formation of yeast had till then been observed. This was now done by Pasteur for that fermentation in which sugar is resolved into lactic acid. This lactic fermentation was at that time brought about by adding some animal substance, such as fibrin, to a solution of sugar, together with chalk that should combine with the acid as it was formed. Pasteur saw, what had never before been noticed, that a fine grey deposit was formed, differing little in appearance from the decomposing fibrin, but steadily increasing as the fermentation proceeded. Struck by the analogy presented by the increasing deposit to the growth of yeast in sweetwort, he examined it with the microscope, and found it to consist of minute particles of uniform size. Pasteur was not a biologist, but although these particles were of extreme minuteness in comparison with the constituents of the yeast plant, he felt convinced that they were of an analogous nature, the cells of a tiny microscopic fungus. This he regarded as the essential ferment, the fibrin or other so-called ferment serving, as he believed, merely

the purpose of supplying to the growing plant certain chemical ingredients not contained in the sugar but essential to its nutrition. And the correctness of this view he confirmed in a very striking manner, by doing away with the fibrin or other animal material altogether, and substituting for it mineral salts containing the requisite chemical elements. A trace of the grey deposit being applied to a solution of sugar containing these salts in addition to the chalk, a brisker lactic fermentation ensued than could be procured in the ordinary way.

I have referred to this research in some detail because it illustrates Pasteur's acuteness as an observer and his ingenuity in experiment, as well as his almost intuitive perception of truth.

A series of other beautiful investigations followed, clearly proving that all true fermentations, including putrefaction, are caused by the growth of micro-organisms.

It was natural that Pasteur should desire to know how the microbes which he showed to be the essential causes of the various fermentations took their origin. It was at that period a prevalent notion, even among many eminent naturalists, that such humble and minute beings originated *de novo* in decomposing organic substances; the doctrine of spontaneous generation, which had been chased successively from various positions which it once occupied among creatures visible to the naked eye, having taken its last refuge where the objects of study were of such minuteness that their habits and history were correspondingly difficult to trace. Here again Pasteur at once saw, as if by instinct, on which side the truth lay; and perceiving its immense importance, he threw himself with ardour into its demonstration. I may describe briefly one class of experiments which he performed with this object. He charged a series of narrow-necked glass flasks with a decoction of yeast, a liquid peculiarly liable to alteration on exposure to the air. Having boiled the liquid in each flask, to kill any living germs it might contain, he sealed its neck with a blow-pipe during ebullition; after which, the flask being allowed to cool, the steam within it condensed, leaving a vacuum above the liquid. If, then, the neck of the flask were broken in any locality, the air at that particular place would rush in to fill the vacuum, carrying with it any living microbes that might be floating in it. The neck of the flask having been again sealed, any germs so introduced would in due time manifest their presence by developing in the clear liquid. When any of such a series of flasks were opened and re-sealed in an inhabited room, or under the trees of a forest, multitudes of minute living forms made their appearance in them; but if this was done in a cellar long unused, where the suspended organisms, like other dust, might be expected to have all fallen to the ground, the decoction remained perfectly clear and unaltered. The oxygen and other gaseous constituents of the atmosphere were thus shown to be of themselves incapable of inducing any organic development in yeast-water.

Such is a sample of the many well-devised experiments by which he carried to most minds the conviction that, as he expressed it, *la génération spontanée est une chimère*, and that the humblest and minutest living organisms can only originate by parentage from beings like themselves.

Pasteur pointed out the enormous importance of these humble organisms in the economy of nature. It is by their agency that the dead bodies of plants and animals are resolved into simpler compounds fitted for assimilation by new living forms. Without their aid the world would be, as Pasteur expresses it, *encombré de cadavres*. They are essential not only to our well-being, but to our very existence. Similar microbes must have discharged the same necessary function of removing refuse and providing food for successive generations of plants and animals during the past periods of the world's history; and it is interesting to think that organisms as simple as can well be conceived to have existed when life first appeared upon our globe have, in all probability, propagated the same lowly but most useful offspring during the ages of geological time.

Pasteur's labours on fermentation have had a very important influence upon surgery. I have been often asked to speak on my share in this matter before a public audience; but I have hitherto refused to do so, partly because the details are so entirely technical, but chiefly because I have felt an invincible repugnance to what might seem to savour of self-advertisement. The latter objection now no longer exists, since advancing years have indicated that it is right for me to leave to younger men the practice of my dearly loved profession. And it will perhaps be

expected that, if I can make myself intelligible, I should say something upon the subject on the present occasion.

Nothing was formerly more striking in surgical experience than the difference in the behaviour of injuries according to whether the skin was implicated or not. Thus, if the bones of the leg were broken and the skin remained intact, the surgeon applied the necessary apparatus without any other anxiety than that of maintaining a good position of the fragments, although the internal injury to bones and soft parts might be very severe. If, on the other hand, a wound of the skin was present communicating with the broken bones, although the damage might be in other respects comparatively slight, the compound fracture, as it was termed, was one of the most dangerous accidents that could happen. Mr. Syme, who was, I believe, the safest surgeon of his time, once told me that he was inclined to think that it would be, on the whole, better if all compound fractures of the leg were subjected to amputation, without any attempt to save the limb. What was the cause of this astonishing difference? It was clearly in some way due to the exposure of the injured parts to the external world. One obvious effect of such exposure was indicated by the odour of the discharge, which showed that the blood in the wound had undergone putrefactive change by which the bland nutrient liquid had been converted into highly irritating and poisonous substances. I have seen a man with compound fracture of the leg die within two days of the accident, as plainly poisoned by the products of putrefaction as if he had taken a fatal dose of some potent toxic drug.

An external wound of the soft parts might be healed in one of two ways. If its surfaces were clean cut and could be brought into accurate apposition, it might unite rapidly and painlessly "by the first intention." This, however, was exceptional. Too often the surgeon's efforts to obtain primary union were frustrated: the wound inflamed and the retentive stitches had to be removed, allowing it to gape; and then, as if it had been left open from the first, healing had to be effected in the other way which it is necessary for me briefly to describe. An exposed raw surface became covered in the first instance with a layer of clotted blood or certain of its constituents, which invariably putrefied; and the irritation of the sensitive tissues by the putrid products appeared to me to account sufficiently for the inflammation which always occurred in and around an open wound during the three or four days which elapsed before what were termed "granulations" had been produced. These constituted a coarsely granular coating of very imperfect or embryonic structure, destitute of sensory nerves and prone to throw off matter or pus, rather than absorb, as freshly divided tissues do, the products of putrefaction. The granulations thus formed a beautiful living plaster, which protected the sensitive parts beneath from irritation, and the system generally from poisoning and consequent febrile disturbance. The granulations had other useful properties of which I may mention their tendency to shrink as they grew, thus gradually reducing the dimensions of the sore. Meanwhile another cause of its diminution was in operation. The cells of the epidermis or scarf-skin of the cutaneous margins were perpetually producing a crop of young cells of similar nature, which gradually spread over the granulations till they covered them entirely, and a complete cicatrix or scar was the result. Such was the other mode of healing, that by granulation and cicatrization; a process which, when it proceeded unchecked to its completion, commanded our profound admiration. It was, however, essentially tedious compared with primary union, while, as we have seen, it was always preceded by more or less inflammation and fever, sometimes very serious in their effects. It was also liable to unforeseen interruptions. The sore might become larger instead of smaller, cicatrization giving place to ulceration in one of its various forms, or even to the frightful destruction of tissue which, from the circumstance that it was most frequently met with in hospitals, was termed hospital gangrene. Other serious and often fatal complications might arise, which the surgeon could only regard as untoward accidents and over which he had no efficient control.

It will be readily understood from the above description that the inflammation which so often frustrated the surgeon's endeavours after primary union was in my opinion essentially due to decomposition of blood within the wound.

These and many other considerations had long impressed me with the greatness of the evil of putrefaction in surgery. I had done my best to mitigate it by scrupulous ordinary cleanliness and the use of various deodorant lotions. But to prevent it

altogether appeared hopeless while we believed with Liebig that its primary cause was the atmospheric oxygen which, in accordance with the researches of Graham, could not fail to be perpetually diffused through the porous dressings which were used to absorb the blood discharged from the wound. But when Pasteur had shown that putrefaction was a fermentation caused by the growth of microbes, and that these could not arise *de novo* in the decomposable substance, the problem assumed a more hopeful aspect. If the wound could be treated with some substance which, without doing too serious mischief to the human tissues, would kill the microbes already contained in it and prevent the future access of others in the living state, putrefaction might be prevented, however freely the air with its oxygen might enter. I had heard of carbolic acid as having a remarkable deodorising effect upon sewage, and having obtained from my colleague Dr. Anderson, Professor of Chemistry in the University of Glasgow, a sample which he had of this product, then little more than a chemical curiosity in Scotland, I determined to try it in compound fractures. Applying it undiluted to the wound, with an arrangement for its occasional renewal, I had the joy of seeing these formidable injuries follow the same safe and tranquil course as simple fractures, in which the skin remains unbroken.

At the same time we had the intense interest of observing in open wounds what had previously been hidden from human view, the manner in which subcutaneous injuries are repaired. Of special interest was the process by which portions of tissue killed by the violence of the accident were disposed of, as contrasted with what had till then been invariably witnessed. Dead parts had been always seen to be gradually separated from the living by an inflammatory process and thrown off as sloughs. But when protected by the antiseptic dressing from becoming putrid and therefore irritating, a structure deprived of its life caused no disturbance in its vicinity; and, on the contrary, being of a nutritious nature, it served as pabulum for the growing elements of the neighbouring living structures, and these became in due time entirely substituted for it. Even dead bone was seen to be thus replaced by living osseous tissue.

This suggested the idea of using threads of dead animal tissue for tying blood-vessels; and this was realised by means of catgut, which is made from the intestine of the sheep. If deprived of living microbes, and otherwise properly prepared, catgut answers its purpose completely; the knot holding securely, while the ligature around the vessel becomes gradually absorbed and replaced by a ring of living tissue. The threads, instead of being left long as before, could now be cut short, and the tedious process of separation of the ligature, with its attendant serious danger of bleeding, was avoided.

Undiluted carbolic acid is a powerful caustic; and although it might be employed in compound fracture, where some loss of tissue was of little moment in comparison with the tremendous danger to be averted, it was altogether unsuitable for wounds made by the surgeon. It soon appeared, however, that the acid would answer the purpose aimed at, though used in diluted forms devoid of caustic action, and therefore applicable to operative surgery. According to our then existing knowledge, two essential points had to be aimed at: to conduct the operation so that on its completion the wound should contain no living microbes, and to apply a dressing capable of preventing the access of other living organisms till the time should have arrived for changing it.

Carbolic acid lent itself well to both these objects. Our experience with this agent brought out what was, I believe, a new principle in pharmacology—namely, that the energy of action of any substance upon the human tissues depends not only upon the proportion in which it is contained in the material used as a vehicle for its administration, but also upon the degree of tenacity with which it is held by its solvent. Water dissolves carbolic acid sparingly and holds it extremely lightly, leaving it free to act energetically on other things for which it has greater affinity, while various organic substances absorb it greedily and hold it tenaciously. Hence its watery solution seemed admirably suited for a detergent lotion to be used during the operation for destroying any microbes that might fall upon the wound, and for purifying the surrounding skin and also the surgeon's hands and instruments. For the last-named purpose it had the further advantage that it did not act on steel.

For an external dressing the watery solution was not adapted, as it soon lost the acid it contained, and was irritating while it lasted. For this purpose some organic substances were found to answer well. Large proportions of the acid could be blended

with them in so bland a form as to be unirritating; and such mixtures, while perpetually giving off enough of the volatile salt to prevent organic development in the discharges that flowed past them, served as a reliable store of the antiseptic for days together.

The appliances which I first used for carrying out the antiseptic principle were both rude and needlessly complicated. The years that have since passed have witnessed great improvements in both respects, of the various materials which have been employed by myself and others, and their modes of application, I need say nothing except to express my belief, as a matter of long experience, that carbolic acid, by virtue of its powerful affinity for the epidermis and oily matters associated with it, and also its great penetrating power, is still the best agent at our disposal for purifying the skin around the wound. But I must say a few words regarding a most important simplification of our procedure. Pasteur, as we have seen, had shown that the air of every inhabited room teems with microbes; and for a long time I employed various more or less elaborate precautions against the living atmospheric dust, not doubting that, as all wounds except the few which healed completely by the first intention underwent putrefactive fermentation, the blood must be a peculiarly favourable soil for the growth of putrefactive microbes. But I afterwards learnt that such was by no means the case. I had performed many experiments in confirmation of Pasteur's germ theory, not indeed in order to satisfy myself of its truth, but in the hope of convincing others. I had observed that uncontaminated milk, which would remain unaltered for an indefinite time if protected from dust, was made to teem with microbes of different kinds by a very brief exposure to the atmosphere, and that the same effect was produced by the addition of a drop of ordinary water. But when I came to experiment with blood drawn with antiseptic precautions into sterilised vessels, I saw to my surprise that it might remain free from microbes in spite of similar access of air or treatment with water. I even found that if very putrid blood was largely diluted with sterilised water, so as to diffuse its microbes widely and wash them of their acrid products, a drop of such dilution added to pure blood might leave it unchanged for days at the temperature of the body, although a trace of the septic liquid undiluted caused intense putrefaction within twenty-four hours. Hence I was led to conclude that it was the grosser forms of septic mischief, rather than microbes in the attenuated condition in which they existed in the atmosphere, that we had to dread in surgical practice. And at the London Medical Congress in 1881, I hinted, when describing the experiments I have alluded to, that it might turn out possible to disregard altogether the atmospheric dust. But greatly as I should have rejoiced at such a simplification of our procedure, if justifiable, I did not then venture to test it in practice. I knew that with the safeguards which we then employed I could ensure the safety of my patients, and I did not dare to imperil it by relaxing them. There is one golden rule for all experiments upon our fellow-men. Let the thing tried be that which, according to our best judgment, is the most likely to promote the welfare of the patient. In other words, Do as you would be done by.

Nine years later, however, at the Berlin Congress in 1890, I was able to bring forward what was, I believe, absolute demonstration of the harmlessness of the atmospheric dust in surgical operations. This conclusion has been justified by subsequent experience: the irritation of the wound by antiseptic irrigation and washing may therefore now be avoided, and nature left quite undisturbed to carry out her best methods of repair, while the surgeon may conduct his operations as simply as in former days, provided always that, deeply impressed with the tremendous importance of his object, and inspiring the same conviction in all his assistants, he vigilantly maintains from first to last, with a care that, once learnt, becomes instinctive, but for the want of which nothing else can compensate, the use of the simple means which will suffice to exclude from the wound the coarser forms of septic impurity.

Even our earlier and ruder methods of carrying out the antiseptic principle soon produced a wonderful change in my surgical wards in the Glasgow Royal Infirmary, which, from being some of the most unhealthy in the kingdom, became, as I believe I may say without exaggeration, the healthiest in the world; while other wards, separated from mine only by a passage a few feet broad, where former modes of treatment were for awhile continued, retained their former insalubrity. This result, I need hardly remark, was not in any degree due to special skill on my

part, but simply to the strenuous endeavour to carry out strictly what seemed to me a principle of supreme importance.

Equally striking changes were afterwards witnessed in other institutions. Of these I may give one example. In the great Allgemeines Krankenhaus of Munich, hospital gangrene had become more and more rife from year to year, till at length the frightful condition was reached that 80 per cent. of all wounds became affected by it. It is only just to the memory of Prof. von Nussbaum, then the head of that establishment, to say that he had done his utmost to check this frightful scourge; and that the evil was not caused by anything peculiar in his management was shown by the fact that in a private hospital under his care there was no unusual unhealthiness. The larger institution seemed to have become hopelessly infected, and the city authorities were contemplating its demolition and reconstruction. Under these circumstances, Prof. von Nussbaum despatched his chief assistant, Dr. Lindpaintner, to Edinburgh, where I at that time occupied the chair of Clinical Surgery, to learn the details of the antiseptic system as we then practised it. He remained until he had entirely mastered them, and after his return all the cases were on a certain day dressed on our plan. From that day forward not a single case of hospital gangrene occurred in the Krankenhaus. The fearful disease pyæmia likewise disappeared, and erysipelas soon followed its example.

But it was by no means only in removing the unhealthiness of hospitals that the antiseptic system showed its benefits. Inflammation being suppressed, with attendant pain, fever, and wasting discharge, the sufferings of the patient were, of course, immensely lessened; rapid primary union being now the rule, convalescence was correspondingly curtailed; while as regards safety and the essential nature of the mode of repair, it became a matter of indifference whether the wound had clean-cut surfaces which could be closely approximated, or whether the injury inflicted had been such as to cause destruction of tissue. And operations which had been regarded from time immemorial as unjustifiable were adopted with complete safety.

It pleases me to think that there is an ever-increasing number of practitioners throughout the world to whom this will not appear the language of exaggeration. There are cases in which, from the situation of the part concerned or other unusual circumstances, it is impossible to carry out the antiseptic system completely. These, however, are quite exceptional; and even in them much has been done to mitigate the evil which cannot be altogether avoided.

I ask your indulgence if I have seemed to dwell too long upon matters in which I have been personally concerned. I now gladly return to the labours of others.

The striking results of the application of the germ theory to surgery acted as a powerful stimulus to the investigation of the nature of the micro-organisms concerned; and it soon appeared that putrefaction was by no means the only evil of microbic origin to which wounds were liable. I had myself very early noticed that hospital gangrene was not necessarily attended by any unpleasant odour; and I afterwards made a similar observation regarding the matter formed in a remarkable epidemic of erysipelas in Edinburgh obviously of infective character. I had also seen a careless dressing followed by the occurrence of supuration without putrefaction. And as these non-putrefactive disorders had the same self-propagating property as ferments, and were suppressed by the same antiseptic agencies which were used for combating the putrefactive microbes, I did not doubt that they were of an analogous origin; and I ventured to express the view that, just as the various fermentations had each its special microbe, so it might be with the various complications of wounds. This surmise was afterwards amply verified. Prof. Ogston, of Aberdeen, was an early worker in this field, and showed that in acute abscesses, that is to say those which run a rapid course, the matter, although often quite free from unpleasant odour, invariably contains micro-organisms belonging to the group which, from the spherical form of their elements, are termed micrococci; and these he classed as streptococci or staphylococci, according as they were arranged in chains or disposed in irregular clusters like bunches of grapes. The German pathologist, Fehleisen, followed with a beautiful research, by which he clearly proved that erysipelas is caused by a streptococcus. A host of earnest workers in different countries have cultivated the new science of Bacteriology, and, while opening up a wide fresh domain of Biology, have demonstrated in so many cases the causal relation between special micro-organisms and special diseases, not

only in wounds but in the system generally, as to afford ample confirmation of the induction which had been made by Pasteur that all infective disorders are of microbic origin.

Not that we can look forward with anything like confidence to being able ever to see the *materies morbi* of every disease of this nature. One of the latest of such discoveries has been that by Pfeiffer of Berlin of the bacillus of influenza, perhaps the most minute of all micro-organisms ever yet detected. The bacillus of anthrax, the cause of a plague common among cattle in some parts of Europe, and often communicated to sorters of foreign wool in this country, is a giant as compared with this tiny being; and supposing the microbe of any infectious fever to be as much smaller than the influenza bacillus as this is less than that of anthrax, a by no means unlikely hypothesis, it is probable that it would never be visible to man. The improvements of the microscope, based on the principle established by my father in the earlier part of the century, have apparently nearly reached the limits of what is possible. But that such parasites are really the causes of all this great class of diseases can no longer be doubted.

The first rational step towards the prevention or cure of disease is to know its cause; and it is impossible to overestimate the practical value of researches such as those to which I am now referring. Among their many achievements is what may be fairly regarded as the most important discovery ever made in pathology, because it revealed the true nature of the disease which causes more sickness and death in the human race than any other. It was made by Robert Koch, who greatly distinguished himself when a practitioner in an obscure town in Germany, by the remarkable combination of experimental acuteness and skill, chemical and optical knowledge and successful micro-photography which he brought to bear upon the illustration of infective diseases of wounds in the lower animals; in recognition of which service the enlightened Prussian Government at once appointed him to an official position of great importance in Berlin. There he conducted various important researches; and at the London congress in 1881 he showed to us for the first time the bacillus of tubercle. Wonderful light was thrown by this discovery upon a great group of diseases which had before been rather guessed than known to be of an allied nature; a precision and efficacy never before possible was introduced into their surgical treatment, while the physician became guided by new and sure light as regards their diagnosis and prevention.

At that same London congress Koch demonstrated to us his "plate culture" of bacteria, which was so important, that I must devote a few words to its description. With a view to the successful study of the habits and effects of any particular microbe outside the living body, it is essential that it should be present unmixed in the medium in which it is cultivated. It can be readily understood how difficult it must have been to isolate any particular micro-organism when it existed mixed, as was often the case, with a multitude of other forms. In fact, the various ingenious attempts made to effect this object had often proved entire failures. Koch, however, by an ingenious procedure converted what had been before impossible into a matter of utmost facility. In the broth or other nutrient liquid which was to serve as food for the growing microbe he dissolved, by the aid of heat, just enough gelatine to ensure that, while it should become a solid mass when cold, it should remain fluid though reduced in temperature so much as to be incapable of killing living germs. To the medium thus partially cooled was added some liquid containing, among others, the microbe to be investigated; and the mixture was thoroughly shaken so as to diffuse the bacteria and separate them from each other. Some of the liquid was then poured out in a thin layer upon a glass plate and allowed to cool so as to assume the solid form. The various microbes, fixed in the gelatine and so prevented from intermingling, proceeded to develop each its special progeny, which in course of time showed itself as an opaque speck in the transparent film. Any one of such specks could now be removed and transferred to another vessel in which the microbe composing it grew in perfect isolation.

Pasteur was present at this demonstration, and expressed his sense of the great progress effected by the new method. It was soon introduced into his own institute and other laboratories throughout the world; and it has immensely facilitated bacteriological study.

One fruit of it in Koch's own hands was the discovery of the microbe of cholera in India, whither he went to study the disease. This organism was termed by Koch from its curved

form the "comma bacillus," and by the French the cholera vibrio. Great doubts were for a long time felt regarding this discovery. Several other kinds of bacteria were found of the same shape, some of them producing very similar appearances in culture media. But bacteriologists are now universally agreed that, although various other conditions are necessary to the production of an attack of cholera besides the mere presence of the vibrio, yet it is the essential *materies morbi*; and it is by the aid of the diagnosis which its presence in any case of true cholera enables the bacteriologist to make, that threatened invasions of this awful disease have of late years been so successfully repelled from our shores. If bacteriology had done nothing more for us than this, it might well have earned our gratitude.

I have next to invite your attention to some earlier work of Pasteur. There is a disease known in France under the name of *cholera des poules*, which often produced great havoc among the poultry yards of Paris. It had been observed that the blood of birds that had died of this disease was peopled by a multitude of minute bacteria, not very dissimilar in form and size to the microbe of the lactic ferment to which I have before referred. And Pasteur found that, if this bacterium was cultivated outside the body for a protracted period under certain conditions, it underwent a remarkable diminution of its virulence; so that, if inoculated into a healthy fowl, it no longer caused the death of the bird, as it would have done in its original condition, but produced a milder form of the disease which was not fatal. And this altered character of the microbe, caused by certain conditions, was found to persist in successive generations cultivated in the ordinary way. Thus was discovered the great fact of what Pasteur termed the *atténuation des virus*, which at once gave the clue to understanding what had before been quite mysterious, the difference in virulence of the same disease in different epidemics.

But he made the further very important observation that a bird which had gone through the mild form of the complaint, had acquired immunity against it in its most virulent condition. Pasteur afterwards succeeded in obtaining mitigated varieties of microbes for some other diseases; and he applied with great success the principle which he had discovered in fowl-cholera for protecting the larger domestic animals against the plague of anthrax. The preparations used for such preventive inoculations he termed "vaccins," in honour of our great countryman, Edward Jenner. For Pasteur at once saw the analogy between the immunity to fowl-cholera produced by its attenuated virus, and the protection afforded against small-pox by vaccination. And while pathologists still hesitated, he had no doubt of the correctness of Jenner's expression *variole vaccine*, or small-pox in the cow.

It is just a hundred years since Jenner made the crucial experiment of inoculating with small-pox a boy whom he had previously vaccinated, the result being, as he anticipated, that the boy was quite unaffected. It may be remarked that this was a perfectly legitimate experiment, involving no danger to the subject of it. Inoculation was at that time the established practice; and if vaccination should prove nugatory, the inoculation would be only what would have been otherwise called for; while it would be perfectly harmless if the hoped-for effect of vaccination had been produced.

We are a practical people, not much addicted to personal commemorations; although our nation did indeed celebrate with fitting splendour the jubilee of the reign of our beloved Queen; and at the invitation of Glasgow the scientific world has lately marked in a manner, though different, as imposing, the jubilee of the life-work of a sovereign in science (Lord Kelvin). But while we cannot be astonished that the centenary of Jenner's immortal discovery should have failed to receive general recognition in this country, it is melancholy to think that this year should, in his native county, have been distinguished by a terrible illustration of the results which would sooner or later inevitably follow the general neglect of his prescriptions.

I have no desire to speak severely of the Gloucester Guardians. They are not sanitary authorities, and had not the technical knowledge necessary to enable them to judge between the teachings of true science and the declamations of misguided, though well-meaning, enthusiasts. They did what they believed to be right; and when roused to a sense of the greatness of their mistake, they did their very best to repair it, so that their city is said to be now the best vaccinated in her Majesty's dominions. But, though by their praiseworthy exertions they succeeded in promptly checking the raging epidemic, they cannot recall the

dead to life, or restore beauty to marred features, or sight to blinded eyes. Would that the entire country and our Legislature might take duly to heart this object-lesson!

How completely the medical profession were convinced of the efficacy of vaccination in the early part of this century, was strikingly illustrated by an account given by Prof. Crookshank, in his interesting history of this subject, of several eminent medical men in Edinburgh meeting to see the (to them) unprecedented fact of a vaccinated person having taken small-pox. It has, of course, since become well known that the milder form of the disease, as modified by passing through the cow, confers a less permanent protection than the original human disorder. This it was, of course, impossible for Jenner to foresee. It is, indeed, a question of degree, since a second attack of ordinary small-pox is occasionally known to occur; and vaccination, long after it has ceased to give perfect immunity, greatly modifies the character of the disorder, and diminishes its danger. And, happily, in re-vaccination after a certain number of years we have the means of making Jenner's work complete. I understand that the majority of the Commissioners, who have recently issued their report upon this subject, while recognising the value and importance of re-vaccination, are so impressed with the difficulties that would attend making it compulsory by legislation that they do not recommend that course; although it is advocated by two of their number who are of peculiarly high authority on such a question. I was lately told by a Berlin professor that no serious difficulty is experienced in carrying out the compulsory law that prevails in Germany. The masters of the schools are directed to ascertain in the case of every child attaining the age of twelve whether re-vaccination has been practised. If not, and the parents refuse to have it done, they are fined one mark. If this does not prove effectual, the fine is doubled; and if even the double penalty should not prove efficacious, a second doubling of it would follow; but, as my informant remarked, it is very seldom that it is called for. The result is that small-pox is a matter of extreme rarity in that country, while it is absolutely unknown in the huge German army, in consequence of the rule that every soldier is re-vaccinated on entering the service. Whatever view our Legislature may take on this question, one thing seems to me clear: that it will be the duty of Government to encourage by every available means the use of calf lymph, so as to exclude the possibility of the communication of any human disease to the child, and to institute such efficient inspection of vaccination institutes as shall ensure careful antiseptic arrangements, and so prevent contamination by extraneous microbes. If this were done, "conscientious objections" would cease to have any rational basis. At the same time, the administration of the regulations on vaccination should be transferred (as advised by the Commissioners) to competent sanitary authorities.

But to return to Pasteur. In 1880 he entered upon the study of that terrible but then most obscure disease, hydrophobia or rabies, which from its infective character he was sure must be of microbic origin, although no micro-organism could be detected in it. He early demonstrated the new pathological fact that the virus had its essential seat in the nervous system. This proved the key to his success in this subject. One result that flowed from it has been the cause of unspeakable consolation to many. The foolish practice is still too prevalent of killing the dog that has bitten any one, on the absurd notion that, if it were mad, its destruction would prevent the occurrence of hydrophobia in the person bitten. The idea of the bare possibility of the animal having been so affected causes an agony of suspense during the long weeks or months of possible incubation of the disease. Very serious nervous symptoms aping true hydrophobia have been known to result from the terror thus inspired. Pasteur showed that if a little of the brain or spinal cord of a dog that had been really mad was inoculated in an appropriate manner into a rabbit, it infallibly caused rabies in that animal in a few days. If, therefore, such an experiment was made with a negative result, the conclusion might be drawn with certainty that the dog had been healthy. It is perhaps right that I should say that the inoculation is painlessly done under an anæsthetic, and that in the rabbit rabies does not assume the violent form that it does in the dog, but produces gradual loss of power with little if any suffering.

This is the more satisfactory because rabbits in which the disease has been thus artificially induced are employed in carrying out what was Pasteur's greatest triumph, the preventive

treatment of hydrophobia in the human subject. We have seen that Pasteur discovered that microbes might under some circumstances undergo mitigation of their virulence. He afterwards found that under different conditions they might have it exalted, or, as he expressed it, there might be a *renforcement du virus*. Such proved to be the case with rabies in the rabbit; so that the spinal cords of animals which had died of it contained the poison in a highly intensified condition. But he also found that if such a highly virulent cord was suspended under strict antiseptic precautions in a dry atmosphere at a certain temperature, it gradually from day to day lost in potency, till in course of time it became absolutely inert. If now an emulsion of such a harmless cord was introduced under the skin of an animal, as in the subcutaneous administration of morphia, it might be followed without harm another day by a similar dose of a cord still rather poisonous; and so from day to day stronger and stronger injections might be used, the system becoming gradually accustomed to the poison, till a degree of virulence had been reached far exceeding that of the bite of a mad dog. When this had been attained, the animal proved incapable of taking the disease in the ordinary way; and more than that, if such treatment was adopted after an animal had already received the poison, provided that too long a time had not elapsed, the outbreak of the disease was prevented. It was only after great searching of heart that Pasteur, after consultation with some trusted medical friends, ventured upon trying this practice upon man. It has since been extensively adopted in various parts of the world with increasing success as the details of the method were improved. It is not of course the case that every one bitten by a really rabid animal takes the disease; but the percentage of those who do so, which was formerly large, has been reduced almost to zero by this treatment, if not too long delayed.

While the intensity of rabies in the rabbit is undoubtedly due to a peculiarly virulent form of the microbe concerned, we cannot suppose that the daily diminishing potency of the cord suspended in dry warm air is an instance of attenuation of virus, using the term "virus" as synonymous with the microbe concerned. In other words, we have no reason to believe that the special micro-organism of hydrophobia continues to develop in the dead cord and produce successively a milder and milder progeny, since rabies cannot be cultivated in the nervous system of a dead animal. We must rather conclude that there must be some chemical poison present which gradually loses its potency as time passes. And this leads me to refer to another most important branch of this large subject of bacteriology, that of the poisonous products of microbes.

It was shown several years ago by Roux and Versin, working in the Institut Pasteur, that the crust or false membrane which forms upon the throats of patients affected with diphtheria contains bacteria which can be cultivated outside the body in a nutrient liquid, with the result that it acquires poisonous qualities of astonishing intensity, comparable to that of the secretion of the poison-glands of the most venomous serpents. And they also ascertained that the liquid retained this property after the microbes had been removed from it by filtration, which proved that the poison must be a chemical substance in solution, as distinguished from the living element which had produced it. These poisonous products of bacteria, or toxins as they have been termed, explain the deadly effects of some microbes, which it would otherwise be impossible to understand. Thus, in diphtheria itself the special bacillus which was shown by Löffler to be its cause, does not become propagated in the blood, like the microbe of chicken cholera, but remains confined to the surface on which it first appeared; but the toxin which it secretes is absorbed from that surface into the blood, and so poisons the system. Similar observations have been made with regard to the microbes of some other diseases, as, for example, the bacillus of tetanus or lockjaw. This remains localised in the wound, but forms a special toxin of extreme potency, which becomes absorbed and diffused through the body.

Wonderful as it seems, each poisonous microbe appears to form its own peculiar toxin. Koch's tuberculin was of this nature, a product of the growth of the tubercle bacillus in culture media. Here, again, great effects were produced by extremely minute quantities of the substance, but here a new peculiarity showed itself, viz. that patients affected with tubercular disease, in any of its varied forms, exhibited inflammation in the affected part and general fever after receiving under the skin an amount of the material which had no effect whatever upon healthy persons. I witnessed in Berlin some

instances of these effects, which were simply astounding. Patients affected with a peculiar form of obstinate ulcer of the face showed, after a single injection of the tuberculin, violent inflammatory redness and swelling of the sore and surrounding skin; and, what was equally surprising, when this disturbance subsided the disease was found to have undergone great improvement. By repetitions of such procedures, ulcers which had previously been steadily advancing, in spite of ordinary treatment, became greatly reduced in size, and in some instances apparently cured. Such results led Koch to believe that he had obtained an effectual means of dealing with tubercular disease in all its forms. Unhappily, the apparent cure proved to be only of transient duration, and the high hopes which had been inspired by Koch's great reputation was dashed. It is but fair to say that he was strongly urged to publish before he was himself disposed to do, and we cannot but regret that he yielded to the pressure put upon him.

But though Koch's sanguine anticipations were not realised, it would be a great mistake to suppose that his labours with tuberculin have been fruitless. Cattle are liable to tubercle, and, when affected with it, may become a very serious source of infection for human beings, more especially when the disease affects the udders of cows, and so contaminates the milk. By virtue of the close affinity that prevails between the lower animals and ourselves, in disease as well as in health, tuberculin produces fever in tubercular cows in doses which do not affect healthy beasts. Thus, by the subcutaneous use of a little of the fluid, tubercle latent in internal organs of an apparently healthy cow can be with certainty revealed, and the slaughter of the animal after this discovery protects man from infection.

It has been ascertained that glanders presents a precise analogy with tubercle as regards the effects of its toxic product. If the microbe which has been found to be the cause of this disease is cultivated in appropriate media, it produces a poison which has received the name of mallein, and the subcutaneous injection of a suitable dose of this fluid into a glandered horse causes striking febrile symptoms which do not occur in a healthy animal. Glanders, like tubercle, may exist in insidious latent forms which there was formerly no means of detecting, but which are at once disclosed by this means. If a glandered horse has been accidentally introduced into a large stable, this method of diagnosis surely tells if it has infected others. All receive a little mallein. Those which become affected with fever are slaughtered, and thus not only is the disease prevented from spreading to other horses, but the grooms are protected from a mortal disorder.

This valuable resource sprang from Koch's work on tuberculin, which has also indirectly done good in other ways. His distinguished pupil, Behring, has expressly attributed to those researches the inspiration of the work which led him and his since famous collaborateur, the Japanese Kitasato, to their surprising discovery of anti-toxic serum. They found that if an animal of a species liable to diphtheria or tetanus received a quantity of the respective toxin, so small as to be harmless, and afterwards, at suitable intervals, successively stronger and stronger doses, the creature, in course of time, acquired such a tolerance for the poison as to be able to receive with impunity a quantity very much greater than would at the outset have proved fatal. So far, we have nothing more than seems to correspond with the effects of the increasingly potent cord in Pasteur's treatment of rabies. But what was entirely new in their results was that, if blood was drawn from an animal which had acquired this high degree of artificial immunity, and some of the clear fluid or serum which exuded from it after it had clotted was introduced under the skin of another animal, this second animal acquired a strong, though more transient, immunity against the particular toxin concerned. The serum in some way counteracted the toxin or was antitoxic. But, more than that, if some of the antitoxic serum was applied to an animal after it had already received a poisonous dose of the toxin, it preserved the life of the creature, provided that too long a time had not elapsed after the poison was introduced. In other words, the antitoxin proved to be not only preventive but curative.

Similar results were afterwards obtained by Ehrlich, of Berlin, with some poisons not of bacterial origin, but derived from the vegetable kingdom; and quite recently the independent labours of Calmette of Lille and Fraser of Edinburgh have shown that antidotes of wonderful efficacy against the venom of serpents may be procured on the same principle. Calmette has obtained

antitoxin so powerful that a quantity of it only a 200,000th part of the weight of an animal will protect it perfectly against a dose of the secretion of the poison glands of the most venomous serpents known to exist, which without such protection would have proved fatal in four hours. For curative purposes larger quantities of the remedy are required, but cases have been already published by Calmette in which death appears to have been averted in the human subject by this treatment.

Behring's darling object was to discover means of curing tetanus and diphtheria in man. In tetanus the conditions are not favourable; because the specific bacilli lurk in the depths of the wound, and only declare their presence by symptoms caused by their toxin having been already in a greater or less amount diffused through the system; and in every case of this disease there must be a fear that the antidote may be applied too late to be useful. But in diphtheria the bacilli very early manifest their presence by the false membrane which they cause upon the throat, so that the antitoxin has a fair chance; and here we are justified in saying that Behring's object has been attained.

The problem, however, was by no means so simple as in the case of some mere chemical poison. However effectual the antitoxin might be against the toxin, if it left the bacilli intact, not only would repeated injections be required to maintain the transient immunity to the poison perpetually secreted by the microbes, but the bacilli might by their growth and extension cause obstruction of the respiratory passages.

Roux, however, whose name must always be mentioned with honour in relation to this subject, effectually disposed of this difficulty. He showed by experiments on animals that a diphtheritic false membrane, rapidly extending and accompanied by surrounding inflammation, was brought to a stand by the use of the antitoxin, and soon dropped off, leaving a healthy surface. Whatever be the explanation, the fact was thus established that the antitoxic serum, while it renders the toxin harmless, causes the microbe to languish and disappear.

No theoretical objection could now be urged against the treatment; and it has during the last two years been extensively tested in practice in various parts of the world, and it has gradually made its way more and more into the confidence of the profession. One important piece of evidence in its favour in this country is derived from the report of the six large hospitals under the management of the London Asylums Board. The medical officers of these hospitals at first naturally regarded the practice with scepticism; but as it appeared to be at least harmless, they gave it a trial; and during the year 1895 it was very generally employed upon the 2182 cases admitted; and they have all become convinced of its great value. In the nature of things, if the theory of the treatment is correct, the best results must be obtained when the patients are admitted at an early stage of the attack, before there has been time for much poisoning of the system; and accordingly we learn from the report that, comparing 1895 with 1894, during which latter year the ordinary treatment had been used, the percentage of mortality, in all the six hospitals combined, among the patients admitted on the first day of the disease, which in 1894 was 22.5, was only 4.6 in 1895; while for those admitted on the second day the numbers are 27 for 1894 and 14.8 for 1895. Thus for cases admitted on the first day the mortality was only one-fifth of what it was in the previous year, and for those entering on the second it was halved. Unfortunately, in the low parts of London, which furnish most of these patients, the parents too often delay sending in the children till much later: so that on the average no less than 67.5 per cent. were admitted on the fourth day of the disease or later. Hence the aggregate statistics of all cases are not nearly so striking. Nevertheless, taking it altogether, the mortality in 1895 was less than had ever before been experienced in those hospitals. I should add that there was no reason to think that the disease was of a milder type than usual in 1895; and no change whatever was made in the treatment except as regards the antitoxic injections.

There is one piece of evidence recorded in the report which, though it is not concerned with high numbers, is well worthy of notice. It relates to a special institution to which convalescents from scarlet fever are sent from all the six hospitals. Such patients occasionally contract diphtheria, and when they do so the added disease has generally proved extremely fatal. In the five years preceding the introduction of the treatment with antitoxin the mortality from this cause had never been less than 50 per cent., and averaged on the whole 61.9 per cent. During

1895, under antitoxin, the deaths among the 119 patients of this class were only 7.5 per cent., or one-eighth of what had been previously experienced. This very striking result seems to be naturally explained by the fact that these patients being already in hospital when the diphtheria appeared, an unusually early opportunity was afforded for dealing with it.

There are certain cases of so malignant a character from the first that no treatment will probably ever be able to cope with them. But taking all cases together it seems probable that Behring's hope that the mortality may be reduced to 5 per cent. will be fully realised when the public become alive to the paramount importance of having the treatment commenced at the outset of the disease.

There are many able workers in the field of Bacteriology whose names time does not permit me to mention, and to whose important labours I cannot refer; and even those researches of which I have spoken have been, of course, most inadequately dealt with. I feel this especially with regard to Pasteur, whose work shines out more brightly the more his writings are perused.

I have lastly to bring before you a subject which, though not bacteriological, has intimate relations with bacteria. If a drop of blood is drawn from the finger by a prick with a needle and examined microscopically between two plates of glass, there are seen in it minute solid elements of two kinds, the one pale orange bi-concave discs, which, seen in mass, give the red colour to the vital fluid, the other more or less granular spherical masses of the soft material called protoplasm, destitute of colour, and therefore called the colourless or white corpuscles. It has been long known that if the microscope was placed at such a distance from a fire as to have the temperature of the human body, the white corpuscles might be seen to put out and retract little processes or pseudopodia, and by their means crawl over the surface of the glass, just like the extremely low forms of animal life termed, from this faculty of changing their form, amoebæ. It was a somewhat weird spectacle, that of seeing what had just before been constituents of our own blood moving about like independent creatures. Yet there was nothing in this inconsistent with what we knew of the fixed components of the animal frame. For example, the surface of a frog's tongue is covered with a layer of cells, each of which is provided with two or more lashing filaments or cilia, and those of all the cells acting in concert cause a constant flow of fluid in a definite direction over the organ. If we gently scrape the surface of the animal's tongue, we can detach some of these ciliated cells; and on examining them with the microscope in a drop of water, we find that they will continue for an indefinite time their lashing movements, which are just as much living or vital in their character as the writhings of a worm. And as I observed many years ago, these detached cells behave under the influence of a stimulus just like parts connected with the body, the movements of the cilia being excited to greater activity by gentle stimulation, and thrown into a state of temporary inactivity when the irritation was more severe. Thus each constituent element of our bodies may be regarded as in one sense an independent living being, though all work together in marvellous harmony for the good of the body politic. The independent movements of the white corpuscles outside the body were therefore not astonishing; but they long remained matters of mere curiosity. Much interest was called to them by the observation of the German pathologist Cohnheim that in some inflammatory conditions they passed through the pores in the walls of the finest blood-vessels, and thus escaped into the interstices of the surrounding tissues. Cohnheim attributed their transit to the pressure of the blood. But why it was that, though larger than the red corpuscles, and containing a nucleus which the red ones have not, they alone passed through the pores of the vessels, or why it was that this emigration of the white corpuscles occurred abundantly in some inflammations and was absent in others, was quite unexplained.

These white corpuscles, however, have been invested with extraordinary new interest by the researches of the Russian naturalist and pathologist, Metchnikoff. He observed that, after passing through the walls of the vessels, they not only crawl about like amoebæ, but, like them, receive nutritious materials into their soft bodies and digest them. It is thus that the effete materials of a tadpole's tail are got rid of; so that they play a most important part in the function of absorption.

But still more interesting observations followed. He found

that a microscopic crustacean, a kind of water-flea, was liable to be infested by a fungus which had exceedingly sharp-pointed spores. These were apt to penetrate the coats of the creature's intestine, and project into its body-cavity. No sooner did this occur with any spore than it became surrounded by a group of the cells which are contained in the cavity of the body and correspond to the white corpuscles of our blood. These proceeded to attempt to devour the spore; and if they succeeded, in every such case the animal was saved from the invasion of the parasite. But if the spores were more than could be disposed of by the devouring cells (phagocytes, as Metchnikoff termed them), the water-flea succumbed.

Starting from this fundamental observation, he ascertained that the microbes of infective diseases are subject to this same process of devouring and digestion, carried on both by the white corpuscles and by cells that line the blood-vessels. And by a long series of most beautiful researches he has, as it appears to me, firmly established the great truth that phagocytosis is the main defensive means possessed by the living body against the invasions of its microscopic foes. The power of the system to produce antitoxic substances to counteract the poisons of microbes is undoubtedly in its own place of great importance. But in the large class of cases in which animals are naturally refractory to particular infective diseases the blood is not found to yield any antitoxic element by which the natural immunity can be accounted for. Here phagocytosis seems to be the sole defensive agency. And even in cases in which the serum does possess antitoxic, or, as it would seem in some cases, germicidal properties, the bodies of the dead microbes must at last be got rid of by phagocytosis, and some recent observations would seem to indicate that the useful elements of the serum may be, in part at least, derived from the digestive juices of the phagocytes. If ever there was a romantic chapter in pathology, it has surely been that of the story of phagocytosis.

I was myself peculiarly interested by these observations of Metchnikoff's, because they seemed to me to afford clear explanation of the healing of wounds by first intention under circumstances before incomprehensible. This primary union was sometimes seen to take place in wounds treated with water-dressing—that is to say, a piece of wet lint covered with a layer of oiled-silk to keep it moist. This, though cleanly when applied, was invariably putrid within twenty-four hours. The layer of blood between the cut surfaces was thus exposed at the outlet of the wound to a most potent septic focus. How was it prevented from putrefying, as it would have done under such influence if, instead of being between divided living tissues, it had been between plates of glass or other indifferent material? Pasteur's observations pushed the question a step further. It now was, How were the bacteria of putrefaction kept from propagating in the decomposable film? Metchnikoff's phagocytosis supplied the answer. The blood between the lips of the wound became rapidly peopled with phagocytes, which kept guard against the putrefactive microbes, and seized them as they endeavoured to enter.

If phagocytosis was ever able to cope with septic microbes in so concentrated and intense a form, it could hardly fail to deal effectually with them in the very mitigated condition in which they are present in the air. We are thus strongly confirmed in our conclusion that the atmospheric dust may safely be disregarded in our operations; and Metchnikoff's researches, while they have illumined the whole pathology of infective diseases, have beautifully completed the theory of antiseptic treatment in surgery.

I might have taken equally striking illustrations of my theme from other departments in which microbes play no part. In fact, any attempt to speak of all that the art of healing has borrowed from science and contributed to it during the past half-century, would involve a very extensive dissertation on pathology and therapeutics. I have culled specimens from a wide field; and I only hope that in bringing them before you I have not overstepped the bounds of what is fitting before a mixed company. For many of you my remarks can have had little, if any, novelty; for others they may perhaps possess some interest as showing that Medicine is no unworthy ally of the British Association; that, while her practice is ever more and more based on science, the ceaseless efforts of her votaries to improve what has been fittingly designated *Que prosunt omnibus artes*, are ever adding largely to the sum of abstract knowledge.

SECTION A.

MATHEMATICS AND PHYSICS.

OPENING ADDRESS BY PROF. J. J. THOMSON, F.R.S.,
PRESIDENT OF THE SECTION.

THERE is a melancholy reminiscence connected with this meeting of our Section, for when the British Association last met in Liverpool the chair in Section A was occupied by Clerk-Maxwell. In the quarter of a century which has elapsed since that meeting, one of the most important advances made in our science has been the researches which, inspired by Maxwell's view of electrical action, confirmed that view, and revolutionised our conception of the processes occurring in the Electro-magnetic field. When the Association last met in Liverpool Maxwell's view was almost without supporters, to-day its opponents are fewer than its supporters then. Maxwell's theory, which is the development and extension of Faraday's, has not only affected our way of regarding the older phenomena of electricity, it has, in the hands of Hertz and others, led to the discovery of whole regions of phenomena previously undreamt of. It is sad to think that his premature death prevented him from reaping the harvest he had sown. His writings are, however, with us, and are a storehouse to which we continually turn, and never, I think, without finding something valuable and suggestive.

"Thus ye teach us day by day,
Wisdom, though now far away."

The past year has been rich in matters of interest to physicists. In it has occurred the jubilee of Lord Kelvin's tenure of the Professorship of Natural Philosophy at the University of Glasgow. Some of us were privileged to see this year at Glasgow an event unprecedented in the history of physical science in England, when congratulations to Lord Kelvin on the jubilee of his professorship were offered by people of every condition and country. Every scientific society and every scientific man is Lord Kelvin's debtor; but no society and no body of men owe him a greater debt than Section A of the British Association; he has done more for this Section than any one else, he has rarely missed its meetings, he has contributed to the Section papers which will make its proceedings imperishable, and by his enthusiasm he has year by year inspired the workers in this Section to renew with increased vigour their struggles to penetrate the secrets of nature. Long may we continue to receive from him the encouragement and assistance which have been so freely given for the past half-century.

By the death of Sir W. R. Grove, the inventor of Grove's cell, we have lost a physicist whose name is a familiar one in every laboratory in the world. Besides the Grove cell, we owe to him the discovery of the gas battery, and a series of researches on the electrical behaviour of gases, whose importance is only now beginning to be appreciated. His essay on the correlation of the physical forces had great influence in promoting that belief in the unity of the various branches of physics which is one of the characteristic features of modern natural philosophy.

In the late Prof. Stoletow, of Moscow, we have lost the author of a series of most interesting researches on the electrical properties of gases illuminated by ultra-violet light, researches which, from their place of publication, are, I am afraid, not so well known in this country as they deserve to be.

As one who unfortunately of late years has had only too many opportunities of judging of the teaching of science in our public and secondary schools, I should like to bear testimony to the great improvement which has taken place in the teaching of physics in these schools during the past ten years. The standard attained in physics by the pupils of these schools is increasing year by year, and great credit is due to those by whose labours this improvement has been accomplished. I hope I may not be considered ungrateful if I express the opinion that in the zeal and energy which is now spent in the teaching of physics in schools, there may lurk a temptation to make the pupils cover too much ground. You may by careful organisation and arrangement ensure that boys shall be taken over many branches of physics in the course of a short time; it is indeed not uncommon to find boys of seventeen or eighteen who have compassed almost the whole range of physical subjects. But although you may increase the rate at which information is acquired, you cannot increase in anything like the same proportion the rate at which the subject is assimilated, so as to become a means of strengthening the mind and a permanent mental endowment when the facts have long been forgotten.

Physics can be taught so as to be a subject of the greatest possible educational value, but when it is so it is not so much because the student acquires a knowledge of a number of interesting and important facts, as by the mental training the study affords in, as Maxwell said, "bringing out theoretical knowledge to bear on the objects and the objects on our theoretical knowledge." I think this training can be got better by going very slowly through such a subject as mechanics, making the students try innumerable experiments of the simplest kind, and what is a matter of importance in school teaching, of the most inexpensive kind, but always endeavouring to arrive at numerical results, rather than by attempting to cover the whole range of mechanics, light, heat, sound, electricity, and magnetism. I confess I regret the presence in examinations intended for school boys of many of these subjects.

I think, too, that in the teaching of physics at our universities, there is perhaps a tendency to make the course too complex and too complete. I refer especially to the training of those students who intend to become physicists. I think that after a student has been trained to take accurate observations, to be alive to those pitfalls and errors to which all experiments are liable, mischief may in some cases be done if, with the view of learning a knowledge of methods, he is kept performing elaborate experiments, the results of which are well known. It is not given to many to wear a load of learning lightly as a flower. With many students a load of learning, especially if it takes a long time to acquire, is apt to crush enthusiasm. Now, there is, I think, hardly any quality more essential to success in physical investigations than enthusiasm. Any investigation in experimental physics requires a large expenditure of both time and patience; the apparatus seldom, if ever, begins by behaving as it ought; there are times when all the forces of nature, all the properties of matter, seem to be fighting against us; the instruments behave in the most capricious way, and we appreciate Court's Trotter's saying, that the doctrine of the constancy of nature could never have been discovered in a laboratory. These difficulties have to be overcome, but it may take weeks or months to do so, and, unless the student is enthusiastic, he is apt to retire disheartened from the contest. I think, therefore, that the preservation of youthful enthusiasm is one of the most important points for consideration in the training of physicists. In my opinion this can best be done by allowing the student, even before he is supposed to be acquainted with the whole of physics, to begin some original research of a simple kind under the guidance of a teacher who will encourage him and assist in the removal of difficulties. If the student once tastes the delights of the successful completion of an investigation, he is not likely to go back, and will be better equipped for investigating the secrets of nature than if, like the White Knight of "Alice in Wonderland," he commences his career knowing how to measure or weigh every physical quantity under the sun, but with little desire or enthusiasm to have anything to do with any of them. Even for those students who intend to devote themselves to other pursuits than physical investigation, the benefits derived from original investigation as a means of general education can hardly be over-estimated, the necessity it entails of independent thought, perseverance in overcoming difficulties, and the weighing of evidence gives it an educational value which can hardly be rivalled. We have to congratulate ourselves that, through the munificence of Mr. Ludwig Mond, in providing and endowing a laboratory for research, the opportunities for pursuing original investigations in this country have been greatly increased.

The discovery at the end of last year by Prof. Röntgen of a new kind of radiation from a highly exhausted tube through which an electric discharge is passing, has aroused an amount of interest unprecedented in the history of physical science. The effects produced *inside* such a tube by the kathode rays, the bright phosphorescence of the glass, the shadows thrown by opaque objects, the deflection of the rays by a magnet, have, thanks to the researches of Crookes and Goldstein, long been familiar to us, but it is only recently that the remarkable effects which occur outside such a tube have been discovered. In 1893, Lenard, using a tube provided with a window made of a very thin plate of aluminium, found that a screen impregnated with a solution of a phosphorescent substance became luminous if placed outside the tube in the prolongation of the line from the kathode through the aluminium window. He also found that photographic plates placed outside the tube in this line were affected, and electrified bodies were discharged; he also ob-

tained by these rays photographs through plates of aluminium or quartz. He found that the rays were affected by a magnet, and regarded them as the prolongations of the kathode rays. This discovery was at the end of last year followed by that of Röntgen, who found that the region round the discharge tube is traversed by rays which affect a photographic plate after passing through substances such as aluminium or cardboard, which are opaque to ordinary light; which pass from one substance to another, without any refraction, and with but little regular reflection; and which are not affected by a magnet. We may, I think, for the purposes of discussion, conveniently divide the rays occurring in or near a vacuum tube traversed by an electric current into three classes, without thereby implying that they are necessarily distinctly different in physical character. We have (1) the kathode rays inside the tube, which are deflected by a magnet; (2) the Lenard rays outside the tube, which are also deflected by a magnet; and (3) the Röntgen rays, which are not, as far as is known, deflected by a magnet. Two views are held as to the nature of the kathode rays; one view is, that they are particles of gas carrying charges of negative electricity, and moving with great velocities which they have acquired as they travelled through the intense electric field which exists in the neighbourhood of the negative electrode. The phosphorescence of the glass is on this view produced by the impact of these rapidly moving charged particles, though whether it is produced by the mechanical violence of the impact, or whether it is due to an electro-magnetic impulse produced by the sudden reversal of the velocity of the negatively charged particle—whether, in fact, it is due to mechanical or electrical causes, is an open question. This view of the constitution of the kathode rays explains in a simple way the deflection of those rays in a magnetic field, and it has lately received strong confirmation from the results of an experiment made by Perrin. Perrin placed inside the exhausted tube a cylindrical metal vessel with a small hole in it, and connected this cylinder with the leaves of a gold-leaf electroscope. The kathode rays could, by means of a magnet, be guided so as either to pass into the cylinder through the aperture, or turned quite away from it. Perrin found that when the kathode rays passed into the cylinder the gold leaf of the electroscope diverged, and had a negative charge, showing that the bundle of kathode rays enclosed by the cylinder had a charge of negative electricity. Crookes had many years ago exposed a disc connected with a gold-leaf electroscope to the bombardment of the kathode rays, and found that the disc received a slight *positive* charge; with this arrangement, however, the charged particles had to give up their charges to the disc if the gold leaves of the electroscope were to be affected, and we know that it is extremely difficult, if not impossible, to get electricity out of a charged gas merely by bringing the gas in contact with a metal. Lord Kelvin's electric strainers are an example of this. It is a feature of Perrin's experiment that since it acts by induction, the indications of the electroscope are independent of the communication of the charges of electricity from the gas to the cylinder, and since the kathode rays fall on the inside of the cylinder, the electroscope would not be affected, even if there were such an effect as is produced when ultra-violet light falls upon the surface of an electro-negative metal when the metal acquires a positive charge. Since any such process cannot affect the total amount of electricity inside the cylinder, it will not affect the gold leaves of the electroscope; in fact, Perrin's experiments prove that the kathode rays carry a charge of negative electricity.

The other view held as to the constitution of the kathode rays is that they are waves in the ether. It would seem difficult to account for the result of Perrin's experiment on this view, and also I think very difficult to account for the magnetic deflection of the rays. Let us take the case of a uniform magnetic field: the experiments which have been made on the magnetic deflection of these rays seem to make it clear that in a magnetic field which is sensibly uniform, the path of these rays is curved; now if these rays were due to ether waves, the curvature of the path would show that the velocity of propagation of these waves varied from point to point of the path. That is, the velocity of propagation of these waves is not only affected by the magnetic field, it is affected differently at different parts of the field. But in a uniform field what is there to differentiate one part from another, so as to account for the variability of the velocity of wave propagation in such a field? The curvature of the path in a uniform field could not be accounted for by supposing that the velocity of this wave motion

depended on the strength of the magnetic field, or that the magnetic field, by distorting the shape of the boundary of the negative dark space, changed the direction of the wave front, and so produced a deflection of the rays. The chief reason for supposing that the kathode rays are a species of wave motion is afforded by Lenard's discovery, that when the kathode rays in a vacuum tube fall on a thin aluminium window in the tube, rays having similar properties are observed on the side of the window outside the tube; this is readily explained on the hypothesis that the rays are a species of wave motion to which the window is partially transparent, while it is not very likely that particles of the gas in the tube could force their way through a piece of metal. This discovery of Lenard's does not, however, seem to me incompatible with the view that the kathode rays are due to negatively charged particles moving with high velocities. The space outside Lenard's tube must have been traversed by Röntgen rays, these would put the surrounding gas in a state in which a current would be readily started in the gas if any electromotive force acted upon it. Now, though the metal window in Lenard's experiments was connected with the earth, and would, therefore, screen off from the outside of the tube any effect arising from slow electrostatic changes in the tube, it does not follow that it would be able to screen off the electrostatic effect of charged particles moving to and from the tube with very great rapidity. For in order to screen off electrostatic effects, there must be a definite distribution of electrification over the screen; changes in this distribution, however, take a finite time, which depends upon the dimensions of the screen and the electrical conductivity of the material of which it is made. If the electrical changes in the tube take place at above a certain rate, the distribution of electricity on the screen will not have time to adjust itself, and the screen will cease to shield off all electrostatic effects. Thus the very rapid electrical changes which would take place if rapidly moving charged bodies were striking against the window, might give rise to electromotive forces in the region outside the window, and produce convection currents in the gas which has been made a conductor by the Röntgen rays. The Lenard rays would thus be analogous in character to the kathode rays, both being convective currents of electricity. Though there are some points in the behaviour of these Lenard rays which do not admit of a very ready explanation from this point of view, yet the difficulties in its way seem to me considerably less than that of supposing that a wave in the ether can change its velocity when moving from point to point in a uniform magnetic field.

I now pass on to the consideration of the Röntgen rays. We are not yet acquainted with any crucial experiment which shows unmistakably that these rays are waves of transverse vibration in the ether, or that they are waves of normal vibration, or indeed that they are vibrations at all. As a working hypothesis, however, it may be worth while considering the question whether there is any property known to be possessed by these rays which is not possessed by some form or other of light. The many forms of light have in the last few months received a noteworthy addition by the discovery of M. Becquerel of an invisible radiation, possessing many of the properties of the Röntgen rays, which is emitted by many fluorescent substances, and to an especially marked extent by the uranium salts. By means of this radiation, which, since it can be polarised, is unquestionably light, photographs through opaque substances similar to, though not so beautiful as, those obtained by means of Röntgen rays, can be taken, and, like the Röntgen rays, they cause an electrified body on which they shine to lose its charge, whether this be positive or negative.

The two respects in which the Röntgen rays differ from light is in the absence of refraction and perhaps of polarisation. Let us consider the absence of refraction first. We know cases in which special rays of the spectrum pass from one substance to another without refraction; for example, Kundt showed that gold, silver, copper allow some rays to pass through them without bending, while other rays are bent in the wrong direction. Pfleger has lately found that the same is true for some of the aniline dyes when in a solid form. In addition to this, the theory of dispersion of light shows that there will be no bending when the frequency of the vibration is very great. I have here a curve taken from a paper by Helmholtz, which shows the relation between the refractive index and the frequency of vibration for a substance whose molecules have a natural period of vibration, and one only; the frequency of this vibration is represented by OK in the diagram. The refractive index increases with the frequency

of the light until the latter is equal to the frequency of the natural vibration of the substance; the refractive index then diminishes, becomes less than unity, and finally approaches unity, and is practically equal to it when the frequency of the light greatly exceeds that of the natural vibration of the molecule. Helmholtz's results are obtained on the supposition that a molecule of the refracting substance consists of a pair of oppositely electrified atoms, and that the specific inductive capacity of the medium consists of two parts, one due to the ether, the other to the setting of the molecules along the lines of electric force.

Starting from this supposition we can easily see without mathematical analysis that the relation between the refractive index and the frequency must be of the kind indicated by the curve. Let us suppose that an electromotive force of given amplitude acts on this mixture of molecules and ether, and let us start with the frequency of the external electromotive force less than that of the free vibrations of the molecules: as the period of the force approaches that of the molecules, the effect of the force in pulling the molecules into line will increase; thus the specific inductive capacity, and therefore the refractive index increases with the frequency of the external force; the effect of the force on the orientation of the molecules will be greatest when the period of the force coincides with that of the molecules. As long as the frequency of the force is less than that of the molecules, the external field tends to make the molecules set so as to increase the specific inductive capacity of the mixture; as soon, however, as the frequency of the force exceeds that of the molecules, the molecules, if there are no viscous forces, will all topple over and point so as to make the part of the specific inductive capacity due to the molecules of opposite sign to that due to the ether. Thus, for frequencies greater than that of the molecules, the specific inductive capacity will be less than unity. When the frequency of the force only slightly exceeds that of the molecules, the effect of the external field on the molecules is very great, so that if there are a considerable number of molecules, the negative part of the specific inductive capacity due to the molecules may be greater than the positive part due to the ether, so that the specific inductive capacity of the mixture of molecules and ether would be negative; no waves of this period could then travel through the medium, they would be totally reflected from the surface.

As the frequency of the force gets greater and greater, its effect in making the molecules set will get less and less, but the waves will continue to be totally reflected until the negative part of the specific inductive capacity due to the molecules is just equal to the positive part due to the ether. Here the refractive index of the mixture is zero. As the frequency of the force increases, its effect on the molecules gets less and less, so that the specific inductive capacity continually approaches that due to the ether alone, and practically coincides with it as soon as the frequency of the force is a considerable multiple of that of the molecules. In this case both the specific inductive capacity and the refractive index of the medium are the same as that of the ether, and there is consequently no refraction. Thus the absence of refraction, instead of being in contradiction to the Röntgen rays, being a kind of light, is exactly what we should expect if the wave length of the light were exceedingly small.

The other objection to these rays being a kind of light is, that there is no very conclusive evidence of the existence of polarisation. Numerous experiments have been made on the difference between the absorption of these rays by a pair of tourmaline plates when their axes are crossed or parallel. Many observers have failed to observe any difference at all between the absorption in the two cases. Prince Galitzine and M. de Karnojitsky, by a kind of cumulative method, have obtained photographs which seem to show that there is a slightly greater absorption when the axes are crossed than there is when the axes are parallel. There can, however, be no question that the effect, if it exists at all, is exceedingly small compared with the corresponding effect for visible light. Analogy, however, leads us to expect that to get polarisation effects we must use, in the case of short waves, polarisers of a much finer structure than would be necessary for long ones. Thus a wire bird-cage will polarise long electrical waves, but will have no effect on visible light. Rubens and Du Bois made an instrument which would polarise the infra-red rays by winding very fine wires very close together on a framework; this arrangement,

however, was too coarse to polarise visible light. Thus, though the structure of the tourmaline is fine enough to polarise the visible rays, it may be much too coarse to polarise the Röntgen rays if these have exceedingly small wave-lengths. As far as our knowledge of these rays extends, I think we may say that though there is no direct evidence that they are a kind of light, there are no properties of the rays which are not possessed by some variety of light.

It is clear that if the Röntgen rays are light rays, their wave-lengths are of an entirely different order to those of visible light. It is perhaps worth notice that on the electro-magnetic theory of light we might expect two different types of vibration if we suppose that the atoms in the molecule of the vibrating substance carried electrical charges. One set of vibrations would be due to the oscillations of the bodies carrying the charges, the other set to the oscillation of the charges on these bodies. The wave-length of the second set of vibrations would be commensurate with molecular dimensions; Can these vibrations be the Röntgen rays? If so, we should expect them to be damped with such rapidity as to resemble electrical impulses rather than sustained vibrations.

If we turn from the rays themselves to the effect they produce, we find that the rays alter the properties of the substances through which they are passing. This change is most apparent in the effects produced on the electrical properties of the substances. A gas, for example, while transmitting these rays is a conductor of electricity. It retains its conducting properties for some little time after the rays have ceased to pass through it, but Mr. Rutherford and I have lately found that the conductivity is destroyed if a current of electricity is sent through the Röntgenised gas. The gas in this state behaves in this respect like a very dilute solution of an electrolyte. Such a solution would cease to conduct after enough electricity had been sent through it to electrolyse all the molecules of the electrolyte. When a current is passing through a gas exposed to the rays, the current destroys and the rays produce the structure which gives conductivity to the gas; when things have reached a steady state the rate of destruction by the current must equal the rate of production by the rays. The current can thus not exceed a definite value, otherwise more of the conducting gas would be destroyed than is produced.

This explains the very characteristic feature that in the passage of electricity through gases exposed to Röntgen rays, the current, though at first proportional to the electromotive force, soon reaches a value where it is almost constant and independent of the electromotive force, and we get to a state when a tenfold increase in the electromotive force only increases the current by a few per cent. The conductivity under the Röntgen rays varies greatly from one gas to another, the halogens and their gaseous compounds, the compounds of sulphur, and mercury vapour, are among the best conductors. It is worthy of note that those gases which are the best conductors when exposed to the rays are either elements, or compounds of elements, which have in comparison with their valency very high refractive indices.

The conductivity conferred by the rays on a gas is not destroyed by a considerable rise in temperature; it is, for example, not destroyed if it be sucked through metal tubing raised to a red heat. The conductivity is, however, destroyed if the gas is made to bubble through water, it is also destroyed if the gas is forced through a plug of glass wool. This last effect seems to indicate that the structure which confers conductivity on the gas is of a very coarse kind, and we get confirmation of this from the fact that a very thin layer of gas exposed to the Röntgen rays does not conduct nearly so well as a thicker one. I think we have evidence from other sources that electrical conduction is a process that requires a considerable space—a space large enough to enclose a very large number of molecules.

Thus Koller found that the specific resistances of petroleum, turpentine, and distilled water, when determined from experiments made with very thin layers of these substances, was very much larger than when determined from experiments with thicker layers. Even in the case of metals there is evidence that the metal has to be of appreciable size if it is to conduct electricity. The theory of the scattering of light by small particles shows that, if we assume the truth of the electro-magnetic theory of light, the effects should be different according as the small particles are insulators or conductors. When the small particles are non-conductors, theory and experiment concur in showing that the direction of complete polarisation for the scattered light is at right angles to the direction of the

incident light, while if the small particles are conductors, theory indicates that the direction of complete polarisation makes an angle of 60° with the incident light. This result is not, however, confirmed by the experiments made by Prof. Threlfall on the scattering of light by very small particles of gold. He found that the gold scattered the light in just the same way as a non-conductor, giving complete polarisation at right angles to the incident light. This would seem to indicate that those very finely divided metallic particles no longer acted as conductors. Thus there seems evidence that in the case of conduction through gases, through badly conducting liquids and through metals, electric conduction is a process which requires a very considerable space and aggregations of large numbers of molecules. I have not been able to find any direct experimental evidence as to whether the same is true for electrolytes. Experiments on the resistance of thin layers of electrolytes would be of considerable interest, as according to one widely-accepted view of electrolysis conduction through electrolytes, so far from being effected by aggregations of molecules, takes place by means of the ion, a structure simpler than that of the molecule, so that if this represents the process of electrolytic conduction, there would not seem room for the occurrence of an effect which occurs with every other kind of conduction.

In this building it is only fitting that some reference should be made to the question of the movement of the ether. You are all doubtless acquainted with the heroic attempts made by Prof. Lodge to set the ether in motion, and how successfully the ether resisted them. It seems to be conclusively proved that a solid body in motion does not set in motion the ether at an appreciable distance outside it; so that if the ether is disturbed at all in such a case, the disturbance is not comparable with that produced by a solid moving through an incompressible fluid, but must be more analogous to that which would be produced by the motion through the liquid of a body of very open structure, such as a piece of wire netting, where the motion of the fluid only extends to a distance comparable with the diameter of the wire, and not with that of the piece of netting. There is another class of phenomena relating to the movement of the ether which is, I think, deserving of consideration, and that is the effect of a varying electro-magnetic field in setting the ether in motion. I do not remember to have seen it pointed out that the electro-magnetic theory of light implicitly assumes that the ether is not set in motion even when acted on by mechanical forces. On the electro-magnetic theory of light such forces do exist, and the equations used are only applicable when the ether is at rest. Consider, for example, the case of a plane electric wave travelling through the ether. We have parallel to the wave-front a varying electric polarisation, which on the theory is equivalent to a current; at right-angles to this, and also in the wave-front, we have a magnetic force. Now, when a current flows through a medium in a magnetic field there is a force acting on the medium at right-angles to the plane, which is parallel both to the current and to the magnetic force; there will thus be a mechanical force acting on each unit volume of the ether when transmitting an electric wave, and since this force is at right-angles to the current and to the magnetic force, it will be in the direction in which the wave is propagated. In the electro-magnetic theory of light, however, we assume that this force does not set the ether in motion, as unless we made this assumption we should have to modify our equations, as the electro-magnetic equations are not the same in a moving field as in a field at rest. In fact, a complete discussion of the transmission of electro-magnetic disturbances requires a knowledge of the constitution of the ether which we do not possess. We now assume that the ether is not set in motion by an electro-magnetic wave. If we do not make this assumption, we must introduce into our equation quantities representing the components of the velocity of the ether, and unless we know the constitution of the ether, so as to be able to deduce these velocities from the forces acting on it, there will be in the equations of the electro-magnetic field more unknown quantities than we have equations to determine. It is, therefore, a very essential point in electro-magnetic theory to investigate whether or not there is any motion of the ether in a varying electro-magnetic field. We have at the Cavendish Laboratory, using Prof. Lodge's arrangement of interference fringes, made some experiments to see if we could detect any movement of the ether in the neighbourhood of an electric vibrator, using the spark which starts the vibrations as the source of light. The movement of the ether, if it exists, will

be oscillatory, and with an undamped vibrator the average velocity would be zero; we used, therefore, a heavily damped vibrator, with which the average velocity might be expected to be finite. The experiments are not complete, but so far the results are entirely negative. We also tried by the same method to see if we could detect any movement of the ether in the neighbourhood of a vacuum-tube emitting Röntgen rays, but could not find any trace of such a movement. Prof. Threlfall, who independently tried the same experiment, has, I believe, arrived at the same conclusion.

Unless the ether is immovable under the mechanical forces in a varying electro-magnetic field, there are a multitude of phenomena awaiting discovery. If the ether does move, then the velocity of transmission of electrical vibrations, and therefore of light, will be affected by a steady magnetic field. Such a field, even if containing nothing but ether, will behave towards light like a crystal, and the velocity of propagation will depend upon the direction of the rays. A similar result would also hold in a steady electric field. We may hope that experiments on these and similar points may throw some light on the properties of that medium which is universal, which plays so large a part in our explanation of physical phenomena, and of which we know so little.

SECTION B.

CHEMISTRY.

OPENING ADDRESS BY DR. LUDWIG MOND, F.R.S.,
PRESIDENT OF THE SECTION.

IN endeavouring to fix upon a suitable theme for the address I knew you would to-day expect from me, I have felt that I ought to give due consideration to the interests which tie this magnificent city of Liverpool, whose hospitality we enjoy this week, to Section B. of the British Association.

I have therefore chosen to give you a brief history of the manufacture of chlorine, with the progress of which this city and its neighbourhood have been very conspicuously and very honourably connected, not only as regards quantity—I believe this neighbourhood produces to-day nearly as much chlorine as the rest of this world together—but more particularly by having originated, worked out, and carried into practice several of the most important improvements ever introduced into this manufacture. I was confirmed in my choice by the fact that this manufacture has been influenced and perfected in an extraordinary degree by the rapid assimilation and application of the results of purely scientific investigations, and of new scientific theories, and offers a very remarkable example of the incalculable value to our commercial interests of the progress of pure science.

The early history of chlorine is particularly interesting, as it played a most important rôle in the development of chemical theories. There can be no doubt that the Arabian alchemist Geber, who lived eleven hundred years ago, must have known that "Aqua Regia," which he prepared by distilling a mixture of salt, nitre, and vitriol, gave off, on heating, very corrosive, evil-smelling, greenish-yellow fumes; and all his followers throughout a thousand years must have been more or less molested by these fumes whenever they used Aqua Regia, the one solvent of the gold they attempted so persistently to produce.

But it was not until 1774 that the great Swedish chemist Scheele succeeded in establishing the character of these fumes. He discovered that on heating manganese with muriatic acid he obtained fumes very similar to those given off by "Aqua Regia," and found that these fumes constituted a permanent gas of yellowish-green colour, very pungent odour, very corrosive, very irritating to the respiratory organs, and which had the power of destroying organic colouring matters.

According to the views prevalent at the time, Scheele considered that the manganese had removed phlogiston from the muriatic acid, and he consequently called the gas dephlogisticated muriatic acid.

When, during the next decade, Lavoisier successfully attacked, and after a memorable struggle completely upset the phlogiston theory, and laid the foundations of our modern chemistry, Berthollet, the eminent "father" of physical chemistry—the science of to-day—endeavoured to determine the place of Scheele's gas in the new theory. Lavoisier was of opinion that all acids, including muriatic acid, contain oxygen. Berthollet found that a solution of Scheele's gas in water, when exposed to the sunlight, gives off oxygen and leaves behind muriatic acid. He

considered this as proof that this gas consists of muriatic acid and oxygen, and called it oxygenated muriatic acid.

In the year 1785 Berthollet conceived the idea of utilising the colour-destroying powers of this gas for bleaching purposes. He prepared the gas by heating a mixture of salt, manganese, and vitriol. He used a solution of the gas in water for bleaching, and subsequently discovered that the product obtained by absorbing the gas in a solution of caustic potash possessed great advantages in practice.

This solution was prepared as early as 1789, at the chemical works on the Quai de Javelle, in Paris, and is still made and used there under the name of "Eau de Javelle."

James Watt, whose great mind was not entirely taken up with that greatest of all inventions—his steam-engine—by which he has benefited the human race more than any other man, but who also did excellent work in chemistry—became acquainted in Paris with Berthollet's process, and brought it to Scotland. Here it was taken up with that energy characteristic of the Scotch, and a great stride forward was made when, in 1798, Charles Tennant, the founder of the great firm, which has only recently lapsed into the United Alkali Company, began to use milk of lime in place of the more costly caustic potash, in making a bleaching liquid; and a still greater advance was made when, in the following year, Tennant proposed to absorb the chlorine by hydrate of lime, and thus to produce a dry substance, since known under the name of bleaching powder, which allowed the bleaching powers of chlorine to be transported to any distance.

In order to give you a conception of the theoretical ideas prevalent at this time, I will read to you a passage from an interesting treatise on the art of bleaching published in 1799 by Higgins. In his chapter "On bleaching with the oxygenated muriatic acid, and on the methods of preparing it," he explains the theory of the process as follows:—

"Manganese is an oxyd, a metal saturated with oxygen gas. Common salt is composed of muriatic acid and an alkaline salt called soda, the same which barilla affords. Manganese has greater affinity to sulphuric acid than to its oxygen, and the soda of the salt greater affinity to sulphuric acid than to the muriatic acid gas; hence it necessarily follows that these two gases (or, rather, their gravitating matter) must be liberated from their former union in immediate contact with each other; and although they have but a weak affinity to one another, they unite in their nascent state, that is to say, before they individually unite to caloric, and separately assume the gaseous state; for oxygen gas and muriatic acid gas already formed will not unite when mixed, in consequence principally of the distance at which their respective atmospheres of caloric keep their gravitating particles asunder. The compound resulting from these two gases still retains the property of assuming the gaseous state, and is the oxygenated muriatic gas."

Interesting as these views may appear, considering the time they were published, you will notice that the rôle played by the manganese in the process, and the chemical nature of this substance, were not at all understood. The law of multiple proportions had not yet been propounded by John Dalton, and the researches of Berzelius on the oxides of manganese were only published thirteen years later, in 1812. The green gas we are considering was still looked upon as muriatic acid, to which oxygen had been added, in contradistinction to Scheele's view, who considered it as muriatic acid, from which something, viz. phlogiston, had been abstracted.

It was Humphry Davy who had, by a series of brilliant investigations, carried out in the laboratory of the Royal Institution between 1808 and 1810, accumulated fact upon fact to prove that the gas hitherto called oxygenated muriatic acid did not contain oxygen. He announced in an historic paper, which he read before the Royal Society on July 12, 1810, his conclusion that this gas was an elementary body, which in muriatic acid was combined with hydrogen, and for which he proposed the name "chlorine," derived from the Greek *χλωρός*, signifying "green," the colour by which the gas is distinguished.

The numerous communications which Humphry Davy made to the Royal Society on this subject form one of the brightest and most interesting chapters in the history of chemistry. They have recently been reprinted by the Alembic Society, and I cannot too highly recommend their study to the young students of our science.

Those who have followed the history of chemistry I need not remind how hotly and persistently Davy's views were combated by a number of the most eminent chemists of his time, led by

Berzelius himself; how long the chlorine controversy divided the chemical world; how triumphantly Davy emerged from it; how completely his views were recognised; and how very instrumental they have been in advancing theoretical chemistry.

The hope, however, which Davy expressed in that same historic paper, "that these new views would perhaps facilitate one of the greatest problems in economical chemistry, the decomposition of the muriates of soda and potash," was not to be realised so soon. Although it had changed its name, chlorine was still for many years manufactured by heating a mixture of salt, manganese, and sulphuric acid in leaden stills, as before.

This process leaves a residue consisting of sulphate of soda and sulphate of manganese, and for some time attempts were made to recover the sulphate of soda from these residues, and to use it for the manufacture of carbonate of soda by the Le Blanc process. On the other hand, the Le Blanc process, which had been discovered and put into practice almost simultaneously with Berthollet's chlorine process, decomposed salt by sulphuric acid, and sent the muriatic acid evolved into the atmosphere, causing a great nuisance to the neighbourhood.

Naturally, therefore, when Mr. William Gossage had succeeded in devising plant for condensing this muriatic acid, the manufacturers of chlorine reverted to the original process of Scheele, and heated manganese with the muriatic acid thus obtained. Since then the manufacture of chlorine has become a bye-product of the manufacture of soda by the Le Blanc process, and remained so till very recently.

For a great many years the muriatic acid was allowed to act upon native ores of manganese in closed vessels of earthenware or stone, to which heat could be applied, either externally or internally. These native manganese ores, containing only a certain amount of peroxide, converted only a certain percentage of the muriatic acid employed into free chlorine, the rest combining with the manganese and iron contained in the ore, and forming a brown and very acid solution, which it was a great difficulty for the manufacturer to get rid of. Consequently, many attempts were made to regenerate peroxide of manganese from these waste liquors, so as to use it over again in the production of chlorine.

These, however, for a long time remained unsuccessful, because the exact conditions for super-oxidising the protoxide of manganese by means of atmospheric air were not yet known.

Meantime, viz. in 1845, Mr. Dunlop introduced into the works created by his grandfather, Mr. Charles Tennant, at St. Rollox, a new and very interesting method for producing chlorine, which was in a certain measure a return to the process used by the alchemists.

Indeed, the first part of this process consisted in decomposing a mixture of salt and nitre with oil of vitriol—a reaction that had been made use of for so many centuries! The chlorine so obtained is, however, not pure, but a mixture of chlorine with oxides of nitrogen and hydrochloric acid, which Mr. Dunlop had to find means to eliminate.

For separating the nitrous oxides, Mr. Dunlop adopted the method introduced twenty years before by the great Gay-Lussac in connection with vitriol-making, viz. absorption by sulphuric acid, and the nitro-sulphuric acid thus formed he also utilised in the same way as that obtained from the towers which still bear Gay-Lussac's illustrious name, viz. by using it in the vitriol process in lieu of nitric acid. He then freed his chlorine gas from hydrochloric acid by washing with water, and so obtained it pure. This process possessed two distinct advantages: (1) it yielded a very much larger amount of chlorine from the same amount of salt, and (2) the nitric acid, which was used for oxidising the hydrogen in the hydrochloric acid, was not lost, because the oxides of nitrogen to which it was reduced answered the purpose for which the acid itself had previously been employed. But this process was very limited in its application, as it could only be worked to the extent to which nitric acid was used in vitriol-making.

The process has been at work at St. Rollox for over fifty years, and, as far as I know, is there still in operation; but I am not aware that it has ever been taken up elsewhere.

Within the last few years, however, several serious attempts have been made to give to this process a wider scope by regenerating nitric acid from the nitro-sulphuric acid and employing it over and over again to convert hydrochloric acid into chlorine. Quite a number of patents have been taken out for this purpose, all employing atmospheric air for reconvertng the nitrous oxides

into nitric acid, and differing mainly in details of apparatus and methods of work, and several of these have been put to practical test on a fairly large scale in this neighbourhood, and also in Glasgow, Middlesbrough, and elsewhere. As I do not want to keep you here the whole afternoon, I have to draw the line somewhere as to what I shall include in this brief history of the manufacture of chlorine, and have had to decide to restrict myself to those methods which have actually attained the rank of manufacturing processes on a large scale. As none of the processes just referred to have attained that position, you will excuse me for not entering into further details respecting them.

Mr. Dunlop's process only produced a very small portion of the chlorine manufactured at that time at St. Rollox, the remainder being made, as before, from native manganese and muriatic acid, leaving behind the very offensive waste liquors I have mentioned before, which increased from year to year, and became more and more difficult to get rid of. The problem of recovering from these liquors the manganese in the form of peroxide Mr. Dunlop succeeded in solving in 1855.

He neutralised the free acid and precipitated the iron present by treating these liquors with ground chalk in the cold and settling out, and in later years, filter-pressing the precipitate, which left him a solution of chloride of manganese, mixed only with chloride of calcium. This was treated with a fresh quantity of milk of chalk, but this time under pressure in closed vessels provided with agitators and heated by steam, under which conditions all the manganese was precipitated as carbonate of manganese. This precipitate was filtered off and well drained, and was then passed on iron trays mounted on carriages through long chambers, in which it was exposed to hot air at a temperature of 300° C., the process being practically made continuous, one tray at the one end being taken out of these chambers, and a fresh tray being put in at the other end. One passage through these chambers sufficed to convert the carbonate of manganese into peroxide, which was used in place of, and in the same way as, the native manganese.

The whole of the residual liquors made at the large works at St. Rollox have been treated by this process with signal success for a long number of years. For a short time the process was discontinued in favour of the Weldon process (of which I have to speak next); but after two years Dunlop's process was taken up again, and, to the best of my knowledge, it is still in operation to this day. It has, however, just like Mr. Dunlop's first chlorine process, never left the place of its birth (St. Rollox), although it was for a period of over ten years without a rival.

In 1866, Mr. Walter Weldon patented a modification of a process proposed by Mr. William Gossage, in 1837, for recovering the manganese that had been used in the manufacture of chlorine. Mr. Gossage had proposed to treat the residual liquors of this manufacture by lime, and to oxidise the resulting protoxide of manganese by bringing it into frequent and intimate contact with atmospheric air. This process—and several modifications thereof subsequently patented—had been tried in various places without success. Mr. Weldon, however, did succeed in obtaining a very satisfactory result, possibly—even probably—because, not being a chemist, he did not add the equivalent quantity of lime to his liquor to precipitate the manganese, but used an excess. However, Mr. Weldon, if he was not a chemist at that time, was a man of genius and of great perseverance. He soon made himself a chemist, and having once got a satisfactory result, he studied every small detail of the reaction with the utmost tenacity until he had thoroughly established how this satisfactory result could be obtained on the largest scale with the greatest regularity and certainty.

He even went further, and added considerably to our theoretical knowledge of the character of manganese peroxide and similar peroxides by putting forward the view that these compounds possess the character of weak acids. He explained in this way the necessity for the presence of an excess of lime or other base if the oxidation of the precipitated protoxide of manganese by means of atmospheric air was to proceed at a sufficiently rapid rate. He pointed out that the product had to be considered as a manganite of calcium, a view which has since been thoroughly proved by the investigations of Goergen and others; and it is only fair to state that Weldon's process is not only a process for recovering the peroxide of manganese originally used, but that he introduced a new substance, viz. manganite of calcium, to be continuously used over and over again in the manufacture of chlorine.

Mr. Weldon had the good fortune that his ideas were taken

up with fervency by Colonel Gamble of St. Helens, and that Colonel Gamble's manager, Mr. F. Bramwell, placed all his experience as a consummate technical chemist and engineer at Mr. Weldon's disposal, and assisted him in carrying his ideas into practice. The result was that a process which many able men had tried in vain to realise for thirty years became in the hands of Mr. Weldon and his coadjutors within a few years one of the greatest successes achieved in manufacturing chemistry.

The Weldon process commences by treating the residual liquor with ground chalk or limestone, thus neutralising the free acid and precipitating any sulphuric acid and oxide of iron present. The clarified liquor is run into a tall cylindrical vessel, and milk of lime is added in sufficient quantity to precipitate all the manganese in the form of protoxide. An additional quantity of milk of lime, from one-fifth to one-third of the quantity previously used, is then introduced, and air passed through the vessel by means of an air-compressor. After a few hours all the manganese is converted into peroxide; the contents of the vessel are then run off; the mud, now everywhere known as "Weldon mud," is settled, and the clear liquor run to waste. The mud is then pumped into large closed stone stills, where it meets with muriatic acid, chlorine is given off, and the residual liquor treated as before.

You note that this process works without any manipulation, merely by the circulation of liquids and thick magmas which are moved by pumping machinery. As compared to older processes it also has the great advantage that it requires very little time for completing the cycle of operations, so that large quantities of chlorine can be produced by a very simple and inexpensive plant. These advantages secured for this process the quite unprecedented success that within a few years it was adopted, with a few isolated exceptions, by every large manufacturer of chlorine in the world; yet it possessed a distinct drawback, viz. that it produced considerably less chlorine from a given quantity of muriatic acid than either native manganese of good quality or Mr. Dunlop's recovered manganese. At that time, however, muriatic acid was produced as a by-product of the Le Blanc process so largely in excess of what could be utilised that it was generally looked upon as a waste product of no value. Mr. Weldon himself was one of the very few who foresaw that this state of things could not always continue. The ammonia soda process was casting its shadow before it. Patented in 1838 by Messrs. Dyar and Hemming it was only after the lapse of thirty years (during which a number of manufacturing chemists of the highest standing had in vain endeavoured to carry it into practice) that this process was raised to the rank of a manufacturing process through the indomitable perseverance of Mr. Earnest Solvay of Brussels, and his clear perception of its practical and theoretical intricacies. A few years later, in 1872, Mr. Weldon already gave his attention to the problem of obtaining the chlorine of the salt used in this process in the form of muriatic acid. He proposed to recover the ammonia from the ammonium chloride obtained in this manufacture by magnesia instead of lime, thus obtaining magnesium chloride instead of calcium chloride, and to produce muriatic acid from this magnesium chloride by a process patented by Clemm in 1863, viz. by evaporating the solution, heating the residue in the presence of steam and condensing the acid vapours given off.

Strange to say, this same method had been patented by Mr. Ernest Solvay within twenty-four years before Mr. Weldon lodged his specification. It has been frequently tried with many modifications, but has never been found practicable. Soon afterwards Mr. Weldon, with the object of reducing the muriatic acid required by his first process, proposed to replace the lime in this process by magnesia, and so to produce a manganite of magnesia. After treating this with muriatic acid and liberating chlorine he proceeded to evaporate the residual liquors to dryness, during which operation all the chlorine they contain would be disengaged as hydrochloric acid and collected in condensers, while the dry residue, after being heated to dull redness in the presence of air, would be reconverted into manganite of magnesia.

This process was made the subject of long and extensive experiments at the works of Messrs. Gamble at St. Helens, but did not realise Mr. Weldon's expectations. It, however, led to some further interesting developments, to which I shall refer later on.

Those of you who were present at the last meeting of the British Association in this city will remember that this Section had the advantage of listening to a paper by Mr. Weldon on his chlorine process, and also to another highly interesting paper by

Mr. Henry Deacon, of Widnes, "on a new chlorine process without manganese." And those of you who came with the then President of the Section (Prof. Roscoe) to Widnes to visit the works of Messrs. Gaskell, Deacon, and Co., will well remember that at these works they saw side by side Weldon's process and Deacon's process in operation, and no one present will have forgotten the thoughtful flashing eyes and impressive face of Mr. Deacon when he explained to his visitors the theoretical views he had formed as regards his process.

Mr. Deacon had made a careful study of thermo-chemistry, which had been greatly developed during the preceding decade by the painstaking, accurate, and comprehensive experiments of Julius Thomsen and of Berthelot, and had led the latter to generalisations, which, although not fully accepted by scientific men, have been of immense service to manufacturing chemistry.

Mr. Deacon came to the conclusion that if a mixture of hydrochloric acid with atmospheric air was heated in the presence of a suitable substance capable of initiating the interaction of these two gases by its affinity to both, it would to a very great extent be converted into chlorine with the simultaneous formation of steam, because the formation of steam from oxygen and hydrogen gives rise to the evolution of a considerably larger quantity of heat than the combination of hydrogen and chlorine. Mr. Deacon found that the salts of copper were a very suitable substance for this purpose, and took out a patent for this process in 1868. He entrusted the study of the theoretical and practical problems connected with this process to Dr. Ferdinand Hurter, who carried them out in a manner which will always remain memorable and will never be surpassed, as an example of the application of scientific methods to manufacturing problems, and which soon placed this beautiful and simple process on a sound basis as a manufacturing operation.

In the ordinary course of manufacture the major part—about two-thirds—of the hydrochloric acid is obtained mixed with air and a certain amount of steam, but otherwise very little contaminated. Instead of condensing the muriatic acid from this mixture of gases by bringing it into contact with water, Mr. Deacon passed it through a long series of cooling pipes to condense the steam, which of course absorbed hydrochloric acid, and formed a certain quantity of strong muriatic acid. The mixture of gases was then passed through an iron superheater to raise it to the required temperature, and thence through a mass of broken bricks impregnated with sulphate or chloride of copper contained in a chamber or cylinder called a decomposer, which was protected from loss of heat by being placed in a brick furnace kept sufficiently hot. In this apparatus from 50 to 60 per cent. of the hydrochloric acid in the mixture of gases was burnt to steam and chlorine. In order to separate this chlorine from the steam and the remaining hydrochloric acid the gases were washed with water, and subsequently with sulphuric acid. The mixture now consisted of nitrogen and oxygen, containing about 10 per cent. of chlorine gas, which could be utilised without any difficulty in the manufacture of bleach liquors and chlorate of potash, and which Mr. Deacon also succeeded in using for the manufacture of bleaching powder, by bringing it into contact in specially constructed chambers with large surfaces of hydrate of lime. Within recent years this latter object has been attained in a more expeditious and perfect manner by continuous mechanical apparatus (of which those constructed by Mr. Robert Hasenclever and Dr. Carl Langer have been the most successful), in which the hydrate of lime is transported in a continuous stream by single or double conveyers in an opposite direction to the current of dilute chlorine, and the bleaching powder formed delivered direct into casks, thereby avoiding the intensely disagreeable work of packing this offensive substance by hand.

Mr. Deacon's beautiful and scientific process thus involves still less movement of materials than the very simple process of Mr. Weldon, because in lieu of large volumes of liquids he only moves a current of gas through his apparatus, which requires a minimum of energy. The only raw material used for converting hydrochloric acid into chlorine is atmospheric air, the cheapest of all at our command. The hydrochloric acid which has not been converted into chlorine by the process is all obtained, dissolved in water, as muriatic acid, and is not lost, as in previous processes, but is still available to be converted into chlorine by other methods, or to be used for other purposes.

In spite of these distinct advantages, this process took a long time before it became adopted as widely as it undoubtedly deserved. This was mainly due to the fact that the economy in

the use of muriatic acid which it effected was at the time when the process was brought out, and for many years afterwards, no object to the majority of chlorine manufacturers, who were still producing more of this commodity than they could use. Moreover, there were other reasons. The plant required for this process, although so simple in principle, is very bulky in proportion to the quantity of chlorine produced, and as I have pointed out, the process only succeeded in converting about one-third of the hydrochloric acid produced into chlorine, the remainder being obtained as muriatic acid, which had in most instances to be converted into chlorine by the Weldon process; so that the Deacon process did not constitute an entirely self-contained method for this manufacture. This defect, of small moment as long as muriatic acid was produced in excessive quantities, was only remedied by an invention of Mr. Robert Hasenclever a short number of years ago; when by the rapid development of the ammonia soda process the previously existing state of things had been completely changed, and when, at least on the continent, muriatic acid was no longer an abundant and valueless bye-product, but, on the contrary, the alkali produced by the Le Blanc process had become a bye-product of the manufacture of chlorine. Mr. Hasenclever, in order to make the whole of the muriatic acid he produces available for conversion into chlorine by the Deacon process, introduces the liquid muriatic acid in a continuous stream into hot sulphuric acid contained in a series of stone vessels, through which he passes a current of air. He thus obtains a mixture of hydrochloric acid and air, well adapted for the Deacon process, the water of the muriatic acid remaining with the sulphuric acid, from which it is subsequently eliminated by evaporation. In this way the chlorine in the hydrochloric acid can be almost entirely obtained in its free state by the simplest imaginable means, and with the intervention of no other chemical agent than atmospheric air. Since their introduction the Deacon process has supplanted the Weldon process in nearly all the largest chlorine works in France and Germany, and is now also making very rapid progress in this country.

Mr. Weldon, when he decided to give up his manganite of magnesia process, by no means relaxed his efforts to work out a chlorine process which should utilise the whole of the muriatic acid. While working with manganite of magnesia he found that magnesia alone would answer the purpose without the presence of the peroxide of manganese. He obtained the assistance of M. Pechiney, of Salindres, and in conjunction with him worked out what has become known as the "Weldon-Pechiney" process, which was first patented in 1884.

This process consists in neutralising muriatic acid by magnesia, concentrating the solution to a point at which it does not yet give off any hydrochloric acid, and then mixing into it a fresh quantity of magnesia so as to obtain a solid oxychloride of magnesium. This is broken up into small pieces, which are heated up rapidly to a high temperature without contact with the heating medium, while a current of air is passing through them. The oxychloride of magnesium containing a large quantity of water, this treatment yields a mixture of chlorine and hydrochloric acid with air and steam, the same as the Deacon process, and this is treated in a very similar way to eliminate the steam and the acid from the chlorine. The acid condensed is, of course, treated with a fresh quantity of magnesia, so that the whole of the chlorine which it contains is gradually obtained in the free state.

The rapid heating to a high temperature of the oxychloride of magnesium without contact with the heating medium was an extremely difficult practical problem, which has been solved by M. Pechiney and his able assistant, M. Boulouvard, in a very ingenious and entirely novel way.

They lined a large wrought-iron box with fire-bricks, and built inside of this vertical fire-brick walls with small empty spaces between them, thus forming a number of very narrow chambers, so arranged that they could all be filled from the top of the box, and emptied from the bottom. These chambers they heated to a very high temperature by passing a gas flame through them, thus storing up in the brick walls enough heat to carry out and complete the decomposition of the magnesium oxychloride, with which the chamber was filled when hot enough.

Mr. Weldon himself called this apparatus a "baker's oven," in which trade certainly the same principle has been employed from time immemorial; but to my knowledge it had never before been used in any chemical industry. This process has been at work at M. Pechiney's large alkali works at Salindres,

and is now at work in this country at the chlorate of potash works of Messrs. Allbright and Wilson, at Oldbury, a manufacture for which it offers special advantages. Mr. Weldon and M. Pechiney had expected that this process would become specially useful in connection with the ammonia soda process by preparing in the way proposed by Mr. Solvay and Mr. Weldon in 1872 a solution of magnesium chloride as a bye-product of this manufacture; but instead of obtaining muriatic acid from this solution by Clemm's process, to treat it by the new process, so as to obtain the bulk of the chlorine at once in the free state. But M. Pechiney did no more succeed than his predecessors in recovering the ammonia by means of magnesia in a satisfactory way.

Quite recently, however, it has been applied to obtain chlorine in connection with the ammonia soda process by Dr. Pick, of Czakowa, in Austria. He recovers the ammonia, as usual, by means of lime, and converts the solution of chloride of calcium, obtained by a process patented by Mr. Weldon in 1869, viz. by treatment with magnesia and carbonic acid under pressure, into chloride of magnesium with the formation of carbonate of lime. The magnesium chloride solution is then concentrated and treated by the Weldon-Pechiney process.

I have repeatedly referred during this brief history to the great change which has been brought about in the position of chlorine manufacture by the development of the ammonia soda process, and have pointed out that the muriatic acid which for a long time was the bye-product of the Le Blanc process, without value, thereby became gradually its main and most valuable product, while the alkali became its bye-product.

I have told you how, very early in the history of this process, Mr. Solvay and Mr. Weldon proposed means to provide for this contingency, and how Mr. Weldon continued to improve these means until the time of his death. Mr. Solvay, on his part, also followed up the subject with that tenacity and sincerity of purpose which distinguishes him; his endeavours being mainly directed to producing chlorine direct from the chloride of calcium running away from his works by mixing it with clay and passing air through the mixture at very high temperatures, thus producing chlorine and a silicate of calcium, which could be utilised in cement-making. The very high temperatures required prevented, however, this process from becoming a practical success.

I have already told you what a complicated series of operations Dr. Pick has lately resorted to in order to obtain the chlorine from this chloride of calcium. Yet the problem of obtaining chlorine as a bye-product of the ammonia soda process presents itself as a very simple one.

This process produces a precipitate of bicarbonate of soda and a solution of chloride of ammonium by treating natural brine, or an artificially made solution of salt, in which a certain amount of ammonia has been dissolved, with carbonic acid. In their original patent of 1838, Messrs. Dyar and Hemming proposed to evaporate this solution of ammonium chloride, and to distil the resulting dry product with lime to recover the ammonia. Now, all that seemed to be necessary to obtain the chlorine from this ammonium chloride was to substitute another oxide for lime in the distillation process, which would liberate the ammonia and form a chloride which, on treatment with atmospheric air would give off its chlorine and reproduce the original oxide. The whole of the reactions for producing carbonate of soda and bleaching powder from salt would thus be reduced to their simplest possible form; the solution of salt, as we obtain it in the form of brine direct from the soil, would be treated with ammonia and carbonic acid to produce bicarbonate and subsequently monocarbonate of soda; the limestone used for producing the carbonic acid would yield the lime required for absorbing the chlorine, and produce bleaching powder instead of being run into the rivers in combination with chlorine in the useless form of chloride of calcium; and both the ammonia (used as an intermediary in the production of soda), and the metallic oxide (used as an intermediary in the production of chlorine), would be continuously recovered.

The realisation of this fascinating problem has occupied me for a great many years. In the laboratory I obtained soon almost theoretical results. A very large number of oxides and even of salts of weak acids were found to decompose ammonium chloride in the desired way; but the best results (as was to be clearly anticipated from thermo-chemical data) were given by oxide of nickel.

When, however, I came to carry this process out on a large

scale, I met with the most formidable difficulties, which it took many years to overcome successfully.

The very fact that ammonium chloride vapour forms so readily metallic chlorides when brought in contact at an elevated temperature with metals or oxides or even silicates, led to the greatest difficulty, viz. that of constructing apparatus which would not be readily destroyed by it.

Amongst the metals we found that platinum and gold were the only ones not attacked at all. Antimony was but little attacked, and nickel resisted very well if not exposed to too high a temperature, so that it could be, and is being, used for such parts of the plant as are not directly exposed to heat. The other parts of the apparatus coming in contact with the ammonium chloride vapour I ultimately succeeded in constructing of cast and wrought iron, lined with fire-bricks or Doulton tiles, the joints between these being made by means of a cement consisting of sulphate of baryta and waterglass.

After means had been devised for preventing the breaking of the joints through the unequal expansion of the iron and the earthenware, the plant so constructed has lasted very well.

Oxide of nickel, which had proved the most suitable material for the process in the laboratory, gave equally good chemical results on the large scale, but occasionally a small quantity of nickel chloride was volatilised through local over-heating, which, however, was sufficient to gradually make up the chlorine conduits. We therefore looked out for an active material free from this objection. Theoretical considerations indicated magnesia as the next best substance, but it was found that the magnesium chloride formed was not anhydrous, but retained a certain amount of the steam formed by the reaction, which gave rise to the formation of a considerable quantity of hydrochloric acid on treatment with hot air. In conjunction with Dr. Eschellman (who carried out the experiments for me), I succeeded in reducing the quantity of this hydrochloric acid to a negligible amount by adding to the magnesia a certain amount of chloride of potassium, which probably has the effect of forming an anhydrous double chloride.

This mixture of magnesia and potassium chloride is, after the addition of a certain quantity of china clay, made into small pills in order to give a free and regular passage throughout their entire mass to the hot air and other gases with which they have to be treated. In order to avoid as far as possible the handling and consequent breaking of these pills, I vapourise the ammonium chloride in a special apparatus, and take the vapours through these pills and subsequently pass hot air through, and then again ammonium chloride vapour, and so on, without the pills changing their place.

The vapourisation of the ammonium chloride is carried out in long cast-iron retorts lined with thin Doulton tiles, and placed almost vertically in a furnace which is kept by producer gas at a very steady and regular temperature. These retorts are kept nearly full with ammonium chloride, so as to have as much active heating surface as possible. From time to time a charge of ammonium chloride is introduced through a hopper at the top of these retorts, which is closed by a nickel plug. The ammonium chloride used is very pure, being crystallised out from its solution as produced in the ammonia soda manufacture by a process patented by Mr. Gustav Jarmay, which consists in lowering the temperature of these solutions considerably below 0°C . by means of refrigerating machinery. The retorts will, therefore, evaporate a very large amount of ammonium chloride before it becomes necessary to take out through a door at their bottom the non-volatile impurities which accumulate in them. The ammonium chloride vapour is taken from these retorts by cast-iron pipes lined with tiles and placed in a brick channel, in which they are kept hot, to prevent the solidification of the vapour, to large upright wrought-iron cylinders which are lined with a considerable thickness of fire-bricks, and are filled with the magnesia pills, which are, from the previous operations, left at a temperature of about 300°C . On its passage through the pills the chlorine in the vapours is completely retained by them, the ammonia and water vapour formed pass on and are taken to a suitable condensing apparatus. The reaction of the ammonium chloride vapour upon magnesia being exo-thermic, the temperature of the pills rises during this operation, and no addition of heat is necessary to complete it. The temperature, however, does not rise sufficiently to satisfactorily complete the second operation, viz. the liberation of the chlorine and the re-conversion of the magnesium chloride into magnesium oxide by means of air. This reaction is slightly endo-thermic, and thus absorbs a small

amount of heat, which has to be provided in one way or another. I effect this by heating the pills to a somewhat higher temperature than is required for the action of the air upon them, viz. to 600°C ., by passing through them a current of a dry inert gas free from oxygen heated by a Siemens-Cowper stove to the required temperature. I use for this purpose the gas leaving the carbonating plant of the ammonia soda process.

This current of gas also carries out of the apparatus the small amount of ammonia which was left in between the pills. It is washed to absorb this ammonia, and after washing, this same gas is passed again through the Siemens-Cowper stove, and thus constantly circulated through the apparatus, taking up the heat from the stove and transferring it to the pills. When these have attained the required temperature, the hot inert gas is stopped and a current of hot air passed through, which has also been heated to 600°C . in a similar stove. The air acts rapidly upon the magnesium chloride, and leaves the apparatus charged with 18 to 20 per cent. of chlorine and a small amount of hydrochloric acid. The chlorine comes gradually down, and when it has reached about 3 per cent. the temperature of the air entering the apparatus is lowered to 350°C . by the admixture of cold air to the hot air from the stove; and the weak chlorine leaving the apparatus is passed through a second stove, in which its temperature is raised again to 600°C ., and passed into another cylinder full of pills which are just ready to receive the hot-air current. A series of four cylinders is required to procure the necessary continuity for the process.

The chlorine gas is washed with a strong solution of chloride of calcium, which completely retains all the hydrochloric acid, and is then absorbed in an apparatus invented by Dr. Carl Langer, by hydrate of lime, which is made to pass by a series of interlocked transporting twin-screws in an opposite direction to the current of gas, and produces very good and strong bleaching powder, in spite of the varying strength of the chlorine gas. The hydrochloric acid absorbed by the solution of calcium chloride can by heating this solution be readily driven out and collected.

This process has now been in operation on a considerable scale at our Works at Winnington for several years, with constantly improving results, notably with regard to the loss of ammonia, which has gradually been reduced to a small amount. The process has fully attained my object, viz. to enable the ammonia soda process to compete, not only in the production of carbonate of soda, but also in the production of bleaching powder, with the Le Blanc process.

Nevertheless, I have hesitated to extend this process as rapidly as I should otherwise have done, because very shortly after I had overcome all its difficulties, entirely different methods from those hitherto employed for the manufacture of chlorine were actively pushed forward in different parts of the globe, for which great advantages were claimed, but the real importance and capabilities of which were and are up to this date very difficult to judge. I refer to the processes for producing chlorine by electrolysis.

During the first decade of this century, Humphry Davy had by innumerable experiments established all the leading facts concerning the decomposing action of an electric current upon chemical compounds. Amongst these he was the first to discover that solutions of alkaline chlorides, when submitted to the action of a current, yield chlorine. His successor at the Royal Institution, Michael Faraday, worked out and proved the fundamental law of electrolysis, known to everybody as "Faraday's Law," which has enabled us to calculate exactly the amount of current required to produce by electrolysis any definite quantity of chlorine. Naturally, since these two eminent men had so clearly shown the way, numerous inventors have endeavoured to work out processes based on these principles for the production of chlorine on a manufacturing scale, but only during the last few years have these met with any measure of success.

It has taken all this time for the classical work of Faraday on electro-magnetism to develop into the modern magneto-electric machine, capable of producing electricity in sufficient quantity to make it available for chemical operations on a large scale; for you must keep in mind that an electric installation sufficient to light a large town will only produce a very moderate quantity of chemicals.

In applying electricity to the production of chlorine, various ways have been followed, both as to the raw materials and as to the apparatus employed. While most inventors have proposed

to electrolyse a solution of chloride of sodium, and to produce thereby chlorine and caustic soda, I am not aware that up to this day any quantity of caustic soda made by electrolysis has been put on to the market.

Only two electrolytic works producing chlorine on a really large scale are in operation to-day. Both electrolyse chloride of potassium, producing as a by-product caustic potash, which is of very much higher value than caustic soda, and of which a larger quantity is obtained for the same amount of current expended. These works are situated in the neighbourhood of Stassfurt, the important centre of the chloride of potassium manufacture. The details of the plant they employ are kept secret, but it is known that they use cells with porous diaphragms of special construction, for which great durability is claimed. There are at this moment a considerable number of smaller works in existence, or in course of erection in various countries, intended to carry into practice the production of chlorine by electrolysis by numerous methods, differing mainly in the details of the cells to be used; but some of them also involving what may be called new principles. The most interesting of these are the processes in which mercury is used alternately as kathode and anode, and salt as electrolyte. They aim at obtaining in the first instance chlorine and an amalgam of sodium, and subsequently converting the latter into caustic soda by contact with water, which certainly has the advantage of producing a very pure solution of caustic soda. Mr. Hamilton Castner has carried out this idea most successfully by a very beautiful decomposing cell, which is divided into various compartments, and so arranged that by slightly rocking the cell the mercury charged with sodium in one compartment passes into another, where it gives up the sodium to water, and then returns to the first compartment, to be recharged with sodium. His process has been at work on a small scale for some time at Oldbury near Birmingham, and works for carrying it out on a large scale are now being erected on the banks of the Mersey, and also in Germany and America.

Entirely different from the foregoing, but still belonging to our subject, are methods which propose to electrolyse the chlorides of heavy metals (zinc, lead, copper, &c.) obtained in metallurgical operations or specially prepared for the purpose, among which the processes of Dr. Carl Hoepfner deserve special attention. They eliminate from the electrolyte immediately both the products of electrolysis, chlorine on one side and zinc and copper on the other, and thus avoid all secondary reactions, which have been the great difficulty in the electrolysis of alkaline chlorides.

All these processes have, however, still to stand the test of time before a final opinion can be arrived at as to the effect they will have upon the manufacture of chlorine, the history of which we have been following, and this must be my excuse for not going into further details. I have endeavoured to give you a brief history of the past of the manufacture of chlorine, but I will to-day not attempt to deal with its future! Yet I cannot leave my subject without stating the remarkable fact that every one of these processes which I have described to you is still at work to this day, even those of Scheele and Berthollet, all finding a sphere of usefulness under the widely varying conditions under which the manufacture of chlorine is carried on in different parts of the world.

Let me express a hope that a hundred years hence the same will be said of the processes now emerging and the processes still to spring out of the inventor's mind. Rapid and varied as has been the development of this manufacture, I cannot suppose that its progress is near its end, and that nature has revealed to us all her secrets as to how to procure chlorine with the least expenditure of trouble and energy. I do not believe that industrial chemistry will in future be diverted from this Section and have to wander to Section A under the agis of applied electricity. I do not believe that the easiest way of effecting chemical changes will ultimately be found in transforming heat and chemical affinity into electricity, tearing up chemical compounds by this powerful medium, and then to recombine their constituents in such form as we may require them. I am sure there is plenty of scope for the manufacturing chemist to solve the problems before him by purely chemical means, of some of which we may as little dream to-day as a few years ago it could have been imagined that nickel would be extracted from its ores by means of carbon-monoxide.

At a meeting of this Association which brings before us an entirely new form of energy, the Röntgen rays, which have

enabled us to see through doors and walls and to look inside the human body; which brings before us a new form of matter, represented by Argon and Helium, which, as their discoverers, Lord Rayleigh and Prof. Ramsay, have now abundantly proved, are certainly elementary bodies, inasmuch as they cannot be split up further, but are not chemical elements, as they possess no chemical affinity and do not enter into combinations—at a meeting at which such astounding and unexpected secrets of nature are revealed to us, who would call in doubt that, notwithstanding the immense progress pure and applied science have made during this century, new and greater and farther-reaching discoveries are still in store for ages to come?

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

THE action of the nominating Committee of the American Association (see NATURE of September 10), in recommending a merely formal meeting next year at Toronto, on the day preceding that of the British Association, evoked a storm of opposition in the general session, and a vote was passed requiring the Council to arrange for a regular meeting and fix the time and place.

The Council subsequently fixed on August 9, 1897, as the time, and Detroit as the place, providing for a recess to Toronto before the final adjournment, in order to welcome the British Association.

Of the various Sections, popular interest evidently centred in that of Social and Economic Science, as was evinced by the very large attendance, and by the attention devoted to its proceedings by the daily press of Buffalo. The first paper read before this section, on "The Monetary Standard," taking strong ground for the gold standard, was read by Wm. H. Hale. Edward Atkinson sent a paper entitled "What is True Money?"; also one entitled "Crime against Labour." Other papers were: "The Competition of the Sexes and its Results," by Lawrence Irwell; "Fashion—a Study," by S. E. Warren; "Citizenship, its Privileges and Duties," "Relics of Ancient Barbarism," and "Practical Studies in Horticulture, Art and Music," by S. F. Kneeland; "An Inheritance for the Waifs," by C. F. Taylor; "The Proposed Sociological Institution," by James A. Skilton; "The Value of Social Settlement," and "The Wages Fund Theory," by A. B. Keeler; "Better Distribution of Forecasts," by John A. Miller; "The Tin-plate Experiment," by A. P. Winston, and "Suicide Legislation," by W. L. O'Neill.

Thirty-five papers were read in the Anthropological Section, including contributions from Brinton, Boas, McGee, Fletcher, Beauchamp, Wright, Mercer, and others. Especial interest was felt in the paper of Secretary F. W. Putnam, on the researches made in the ancient city of Coapan, located in Honduras, just over the border from Guatemala.

Prof. Putnam was the first to go beneath the surface. He began in Yucatan, and soon found that buildings now on the surface were of recent date; but underneath were indications of remote antiquity. City has been built over city, in one place as many as five having been superposed, showing as many successive occupations.

The two Biological Sections had twenty-three papers in zoology and forty-two in botany. Among the well known contributors were L. O. Howard, E. D. Cope, L. M. Underwood, T. N. Gill, D. S. Kellicott, C. E. Bessey, J. M. Coulton, and N. L. Britton. The Botanical Club also held several meetings, and the botanists devoted all day Friday to an excursion by lake to Point Abino.

The Geological Section was enriched by all the papers from the Geological Society of America, which held merely a business meeting, an arrangement now adopted for the first time, but so successfully that it will be extended next year to the Chemical Society, and ultimately to other affiliated societies. Thirty-four papers were read to the geologists, among prominent contributors being B. K. Emerson, Warren Upham, I. C. White, E. W. Claypole, G. K. Gilbert, and J. W. Spencer. H. O. Hovey, who has made a speciality of cave explorations, gave interesting accounts of new discoveries in Mammoth Cave and elsewhere. The feature of this Section was the commemorative exercises on Wednesday afternoon, referring to the sixtieth anniversary of the work of Prof. James Hall in connection with the survey of New York State. Addresses and papers were given by Prof. Emerson, Prof. Joseph Le Conte, W. J. McGee,

John M. Clarke, and others. Three of those present at that gathering had attended the meeting of the American Association for the Advancement of Science at Albany in 1856, at which Prof. Hall presided, and which was the largest scientific gathering up to that time held in America. They were Joseph Le Conte, Thomas H. Feary, and Wm. H. Hale.

The proximity of Niagara, and the new applications of power, gave special interest to the Section of Mechanical Science and Engineering, and twenty-two papers were read. Henry T. Eddy, Thomas Gray, J. E. Denton, D. S. Jacobus, and Octave Chanute were among the contributors. A most important paper was read by Elmer L. Corthell, entitled "Some Notes, Physical and Commercial, upon the Delta of the Mississippi River." Mr. Corthell has made a special study of the Mississippi for many years. He points out certain measures where he pronounces necessary to preserve navigation of the delta. The United States has already expended thirty-eight million dollars in the improvement of the Mississippi.

The Chemical Section was crowded with papers, about seventy being read. Among the contributors were A. A. Noyes, A. B. Prescott, H. W. Wiley, R. B. Warder, F. W. Clarke, T. H. Norton, C. B. Dudley, W. P. Mason, J. L. Howe, C. F. Mabery, H. A. Weber, E. W. Hilyard, A. R. Leeds, Wm. McMurtrie, L. L. Van Slyke, and E. A. de Schweinitz. The papers were mostly technical, and were arranged in groups according to the subjects. The programme of the American Chemical Society, which met in the preceding week, was also a long one, indicating an unusual interest in chemistry. At the meeting Prof. Dennis stated that he had found potassium platino-cyanide, $K_2Pt(CN)_6$, by far the best material for painting fluorescent screens for X-ray investigation.

Physics also aroused much interest, and it was remarked that the Section had never had a better programme. Of the thirty-two papers presented, Wm. A. Rogers read five. In one of these he maintained that X-ray pictures could be obtained by the use of static electricity, and he exhibited several pictures taken in that manner. Among others, papers were read by Ernest Merritt, Edward L. Nichols, and Alexander Macfarlane.

The Section of Mathematics and Astronomy was the lightest of all, having only ten papers and no presidential address, Mr. Wm. E. Story being absent in consequence of sickness in his family. Alexander Macfarlane was elected vice-president in his place. G. W. Hough contributed a paper on motion of the great red spot and equatorial belt of the planet Jupiter from 1879 to 1896, and L. A. Bauer one on component fields of the earth's magnetism.

The evening addresses before the Association were by J. W. Spencer, on "Niagara as a time-piece," and by E. D. Cope, on "The results of cave explorations in the United States, and their bearing on the antiquity of man." Spencer's last estimate of the age of Niagara is 31,500 years. In about 5000 years he predicts that the elevation of the north-east will suffice to turn the drainage of the great lakes into the Mississippi River. Prof. Cope gave an exhaustive review of cave explorations.

Contributions to the monument to Pasteur were solicited from the Association, but funds were not available, except from Mrs. Esther Herrman, a patron of the Association, who contributed 100 dollars for that purpose. Grants for research were only made to the extent of 200 dollars, for the same reason; and were allocated as follows:—To the Marine Biological Laboratory, Woods Holl, Mass., for a table (appointment to be made by the vice-presidents for Sections F and G and the director of the laboratory), 100 dollars; to Francis E. Phillips, for investigations on the properties of natural gas, 50 dollars; to L. A. Bauer for investigations on terrestrial magnetism in connection with the magnetic survey of Maryland, 50 dollars.

The President and Vice-Presidents of the next meeting are:—President, Wolcott Gibbs. Vice-Presidents: (A) Mathematics and Astronomy, W. W. Beman; (B) Physics, Carl Barus; (C) Chemistry, W. P. Mason; (D) Mechanical Science and Engineering, John Galbraith; (E) Geology and Geography, I. C. White; (F) Zoology, G. Brown Goode; (G) Botany, George F. Atkinson; (H) Anthropology, W. J. McGee; (I) Social and Economic Science, Richard T. Colburn.

An unusually large number of Fellows were elected, among whom must be mentioned Wolcott Gibbs, he having been elected honorary fellow in order to qualify for the presidency of the Association, of which he had not been a member for nearly thirty years.

The matter of the approaching jubilee (in 1898) of the As-

sociation was discussed, but no definite decision was arrived at. As the probable place of meeting that year, Secretary Putnam suggested Boston, a city already memorable in the annals of the Association as the place where the largest meeting of members—not counting foreign guests—was held.

THE RECENT CYCLONE IN PARIS.

THERE seems to be very little doubt that Paris on Thursday last was visited by a tornado, the first time within the memory of man. It was accompanied by that mysterious circular motion that is special to this class of storm, and extended over a very small area, beginning at the Place St. Sulpice and ending at the Boulevard de la Villette, a distance of nearly two miles. It, however, caused considerable damage, resulting in, it is said, seven deaths and many severe injuries. On the day in question there had been since noon a succession of showers, and it was towards the last of these—about 3 p.m.—that the tornado showed itself. M. Angot, head of the Meteorological Bureau, was at the Pont Royal, about to take a boat, when he noticed small dark clouds, very low down, apparently moving against the wind, which was not at all high, the velocity not being more than five or six yards a second. He soon, however, perceived that the clouds had a rapid circular motion, not horizontal, but oblique. When making these observations he judged the distance of the storm to be about a mile, and its diameter about 170 yards. At the Tour St. Jacques, the meteorologist there states that the storm lasted less than a minute. Some black clouds passed swiftly overhead, and there was one flash of lightning. The barometer suddenly fell from 748 mm. to 742 mm., a drop of 6 mm.; a fact unprecedented for years, but almost immediately afterwards rose again. Advancing from this point towards the north-east, branches and, in some cases, whole trees fell on the roadways, and boats on the river were torn from their moorings and dashed on the quays. Omnibuses were upset, cabs thrown about, and stalls overturned. So strong was the force of the wind that the Palais de Justice had its windows broken and was partly unroofed. The roofs of the Opéra Comique, the Châtelet, the Tribunal of Commerce, and the Préfecture of Police were considerably damaged, and in some cases partly removed. Owing to the great damage done to the numerous windows of every house, the streets were strewn with enormous quantities of glass broken into small pieces. Some curious instances are related. A kiosk in front of the Ambigu, in which were seated two policemen, was carried, together with the policemen, to the other side of the street; the kiosk was completely wrecked, but the policemen were unhurt though shaken. The heavy rain which continued during the storm did considerable damage, filling up cellars, &c., and flooding the river Bièvre. It was owing, perhaps, to this rain, which had cleared the streets of people, that the number of accidents was not greater than was recorded.

We have received the following further details from a correspondent in Paris:—

"The storm which we experienced took meteorologists quite by surprise, and it was found impossible to follow the track of the cyclone out of Paris. It appears that it developed at the Place St. Sulpice, and disappeared at La Villette, seven kilometres in the north-north-east direction.

"The path of destruction was limited to about one hundred yards, but omnibuses were overturned, boats on the Seine wrecked, five persons killed, seventy wounded, and about 100 trees uprooted. One of the most extraordinary places of devastation was the Square de la Tour Saint Jacques, where the Central Municipal Observatory is established. The branches of trees accumulated by the wind were so numerous that I was obliged to use ladders for visiting the observers, who were practically prisoners in the observatory. Most interesting observations were taken from the top of the Eiffel Tower; these will be discussed in the forthcoming International Congress of Meteorology."

In a later communication our correspondent says:—

"A singular observation was registered on the barometer at 2h. 40m. p.m. on the 10th, when the storm raged in Paris. A rise of 1 mm. of mercury was registered, but of such a short duration that it was hardly possible to detect the two separate strokes for the greater part of the variation. (It may here be

stated that the instrument was a self-recording one, the mode of registration being graphical.) A similar aerial commotion was registered at the Tour St. Jacques; but, instead of marking an increase of pressure, the trace showed a depression of 6 mm., and of very short duration."

PREHISTORIC EUROPEAN ART.

IT is important to determine how far culture can independently arise in a given district, and how far it is dependent upon other centres of civilisation. For many years M. Salomon Reinach has devoted himself to these problems, especially in reference to the culture of prehistoric Europe. In his essays on "Le Mirage Orientale" he opposed the very prevalent idea that all our culture necessarily came from the East, and during the last three years he has contributed to *L'Anthropologie* a series of articles on "Sculpture in Europe before the Greco-Roman Influences." This long series of papers is concluded in the current number (No. 2, vol. vii.) of that journal, and it forms a mine of information which cannot but prove of immense value to archaeological students, especially as it is illustrated with 441 outline sketches culled from a vast array of authors. His general thesis comprises two arguments—the one negative, the other positive.

(1) M. Reinach tries to prove that the most primitive European artistic remains are far from justifying the view that the first models and tentative efforts came from Egypt or Babylon. One cannot trace any imitation of Assyrian cylinders or of Egyptian funeral figurines. The fauna figured by the rude artists of Europe is purely European; there is no lion, panther, or camel. An apparently very grave difficulty occurs in the series of figures representing nude females, which authors agree in regarding as imitations of the Babylonian Astarte. M. Reinach argues that this type was indigenous, and so far from owing its existence to Babylonian influence, it, on the contrary, worked its way, in all probability, towards the valleys of the Euphrates and Tigris. He thinks that Europe (*i.e.* the Balkan Peninsula, the Archipelago, the Caucasus, and the west coast of Asia Minor) only later, and to a restricted degree, became dependent upon the old civilisations of the Orient. In his opinion culture is polygenist. He admits multiple centres of creation for art, and refuses to believe that all illumination has come to us from the Euphrates and the Nile. He thinks that the Danube and the Rhine have some rights which should not be neglected, and that the future barbarians who dwelt along the borders of these rivers were not reduced to receive everything from without.

(2) M. Reinach recognises that it is not sufficient to affirm that art can be born in diverse places, and that the germ has not arisen from two or three privileged centres of the ancient world; and so he sets himself to show how the rudiment of art has been able to arise, even among peoples whose genius was for a long time in abeyance. To that purpose M. Reinach has "insisted on the evolution of the most simple decorative motives which, at a certain point, quite naturally suggested the idea of the human or animal form." In these not very numerous cases one can follow the transformations of a plastic motive down to the entirely geometric figure from which it arose. But the taste for geometric forms and the tendency to conventionalisation (*stylisation*), that is to say, to the purely decorative modification of organic lines, have been, for long centuries, so powerful in Europe, that even foreign types have not escaped their petrifying action. *A fortiori*, the indigenous types, arisen from geometrical devices, have always been constrained to return back to them again. It is not denied that in Europe, as elsewhere, the imitation of surrounding nature has given origin to some plastic attempts; but there is proof that this inspiration drawn from nature has been feeble, even in the imitation of animal forms, which represented only a very small number of the animals known to the people."

The author admits that several statuettes figured in this memoir reflect outside influences, particularly of Italy, where Ionian art early took root. But these influences were not exercised in an immediate manner, and the indigenous style appears to have always been predominant even when brought face to face with foreign objects. A similar phenomenon is noticeable in Italy itself, which was Hellenised very slowly, and was only partially Orientalised under the Roman empire.

Such is an outline of M. Reinach's position. There is no

doubt that it will open up a wide discussion, as he covers a great deal of ground, and deals with some matters which admit of diversity of opinion.

M. Reinach, in an earlier section of his memoir (*L'Anth. v.*, 1894, p. 305), definitely states that "in the primitive art of Central Europe the geometric form (a triangle) has suggested the anthropomorphic form, and it is not the anthropomorphic figure which is degenerated into the geometric." Possibly some, at all events, of these flat plates had indications of features painted on their surface, and thus they may have been more realistic than now appears, and later they were made more human-like as the fabricators became more skilled, or as they valued greater realism.

The investigations of quite a number of men of science show that so-called "geometric" designs are often really highly conventionalised representations of natural objects, mainly of animals; others are suggestions of textiles, or other handicrafts. Probably relatively few "geometric" designs are purely meaningless decorations. So far as available evidence goes, there are not many (if any) examples of the evolution of human or animal forms by "suggestion" from purely geometric designs, but the reverse process is extremely common. Doubtless some of the problems involved in this memoir will be fully discussed at the forthcoming meeting of the British Association at Liverpool during the great discussion, which has been arranged for, on the culture and origins of the Mediterranean race. We understand that M. Reinach intends to be present on this occasion, when he will be able to state his views and reply to his critics.

NOTES.

THE seventh annual general meeting of the Federated Institution of Mining Engineers began, with a good attendance, at Cardiff on Tuesday last, under the presidency of Mr. G. A. Mitchell. The report of the Council showed satisfactory progress. It was announced that Mr. Lindsay Wood has been elected President of the Institution.

THE third annual congress of Sunday Societies is announced to take place at Newcastle-on-Tyne, on October 10 and two following days. Copies of the programme of proceedings may be had of the Honorary Secretary, Mr. Mark H. Judge, 7 Pall Mall, S.W.

A REUTER dispatch from Naples says the death is announced of Senator Palmieri, Director of the Vesuvius Observatory. Luigi Palmieri was born in 1807. He was successively Professor of Mathematics at Salerno, Campobasso, and Avellino, Professor of Physics at the Royal Naval School at Naples, and Professor at the University in the same town. In 1854 he was appointed Director of the Vesuvius Meteorological Observatory. He was inventor of several instruments for the observation of natural phenomena, including an electrometer for ascertaining the amount of electricity in the atmosphere, a rain gauge, and a seismometer.

PROF. J. C. BOSE, of the Presidency College, Calcutta, is at present in this country, having been deputed by the Indian Government to visit the various laboratories in Great Britain and on the continent, with a view to the extension of the Calcutta Presidency College Laboratory, and the establishment of a new magnetic observatory in connection with that College. Prof. Bose is the holder of a Royal Society grant for researches in regard to electricity. He is a D.Sc. of London University.

It was announced at a banquet given to Dr. Nansen at Christiania, on Thursday last, that a Nansen fund had been formed for the advancement of science. Subscriptions to the amount of 210,000 kroner had already been received.

THE Russian Geographical Society has been asked by the Governor-General of Turkestan to send some men of science to Shignan and Koshan next summer, for the purpose of making a thorough exploration of those regions.

M. MOUREAUX, who has just returned from Russia, has made observations of some quite surprising magnetical perturbations. On August 29, special perturbations were observed by him of the same kind that are generally registered when connected with earthquakes. He remained several days supposing that he had been mistaken, when he learned through a steamer which arrived that Hecla had been in eruption on that same night. The exact times of occurrence of these perturbations were 11h. 36m., 11h. 42m., 11h. 46m. p.m. Paris mean time.

PROF. H. MOHN informs us that Captain Salvesen, Royal Norwegian Navy, commanding the sloop-of-war *Ellida*, on July 28, when off the promontory of Stat, on the west coast of Norway, saw the "blue sun" at sunset. The phenomenon was seen twice, the ship being lifted on the waves, with an interval of a few seconds of time. The sun was quite clear at setting. It is curious that this beautiful sight is so rarely looked for.

It is reported that the War Department of the United States has sent to Paris for a set of the Bertillon instruments, and that a thorough examination into the system of identification is to be made with a view to its introduction into the United States Army.

THE *Evening Post* of New York learns that Prof. C. W. Dodge, of the Biological Department of the University of Rochester, has asked the trustees to make an appropriation for the establishment of a biological laboratory at Hemlock Lake, a small lake thirty miles south of Rochester, from which the city obtains its water-supply. Prof. Dodge proposes to make, with the assistance of students from his department, a complete biological survey of the lake.

PROF. A. HALL, JUN., Director of the University of Michigan Observatory, according to *Popular Astronomy*, has secured an extensive series of observations of Polaris for latitude variation. He is also engaged on the division errors of the meridian circle, no examination of the errors having been made for some considerable time.

MR. E. H. PARKER, who has been for the past fifteen years in the experimental department of Messrs. Wm. Denny and Bros., Leven Ship-yards, has been appointed Secretary to the Institution of Engineers and Shipbuilders in Scotland, in the place of Mr. W. D. Millar, who is retiring after twenty-five years' service.

WE have received from Colonel A. T. Fraser, R.E. (retired list), two radiographs, one of a European, and the other of a Hindu hand, taken by means of Röntgen rays under precisely similar conditions. Colonel Fraser is of opinion that the evident difference in appearance suggests that the skin pigment of the hand of the Hindu intercepts the rays.

THE Society of Medical Phonographers, to which we have referred on former occasions, has made steady progress during the past year, and now has 250 names on its books. The Society issues a monthly medical periodical, entitled the *Record*, in lithographed shorthand; in addition to this publication it has brought out two small pamphlets dealing with the use of shorthand by the student and by the practitioner respectively, and has issued a list of more than 2500 phonographic outlines of medical terms. The Society intends, at the end of October or the beginning of November, to hold an examination in shorthand for students of medicine commencing their first winter session. Prizes will be given for proficiency in the art.

A LIST of free popular lectures to be delivered in the Chemical Theatre of Owens College, Manchester, on Saturday afternoons during the session, has been sent us. On October 24 will

be begun the first of three discourses on "The Geological History of the District round Manchester," by Prof. Boyd Dawkins, and on subsequent dates the following lectures will be given:—"The Inhabitants of the Seas," by Prof. Hickson; "Economic Botany," by Prof. Weiss; "Soils, their Nature and Origin," by Dr. Burghardt; "Birds," by Mr. Hoyle. In addition to these lectures, others of a popular character will be delivered on Monday evenings during the session. In the list we notice one by the Archdeacon of Manchester, on "Falling Stars as a Branch of Astronomy"; one on "The Electric Furnace," by Prof. Roberts-Austen; and two, by Prof. H. B. Dixon, on "The Nature of Flame."

THE *Pioneer Mail* (Allahabad) on August 19 contains an account of the proceedings at the last meeting of the Central Committee of the Pasteur Institute of India. At this meeting it was unanimously decided "that the scope of the Institute as embodied in the extended scheme laid before the meeting be accepted and published." The scheme in question is as follows: I. The practical application of bacteriological methods to the prevention and cure of disease, viz.: (a) Inoculations against rabies. (b) Preparation and preservation of cholera vaccines for distribution when necessary, and the carrying out of anti-cholera inoculations. (c) Preparation of diphtheria anti-toxin. (d) Preparation of anthrax vaccine. (e) Preparation of tuberculin (for diagnostic purposes). (f) Preparation of mallein (for diagnostic purposes). (g) Vaccination against tetanus. II. The investigation of tropical diseases especially prevalent in India; that is to say: (a) Research intended to generalise methods already in use, and to test the actual value of proposed methods, namely: (1) Vaccination against enteric fever. (2) The use of *anti-venene* in snake-bit. (3) The curative treatment of cholera. (b) The investigation of the following diseases: malaria, kala azar, dysentery, abscess of the liver, beri beri, &c. (c) Fermentations, including indigo, opium, tea. III. The provision of a centre which would afford to medical officers, already trained in bacteriological technique, the means for the prosecution of independent research, and for the acquisition of advanced knowledge of bacteriological methods of dealing with disease, under the guidance and supervision of the officers of the Institute. The selection of the site for the Institute has yet to be made.

WE are glad to hear that the Crystal Palace Company are about to arrange for a series of entertainments and lectures during the ensuing autumn and winter months, to be given every Wednesday evening. The lectures will be devoted to the exemplification of the various great discoveries and inventions of the Victorian era, and will be delivered by eminent men of science. We wish the Crystal Palace every success in this venture, and hope that the Company's example may be followed by others. The last half-century, perhaps, holds the record for the greatest advancement in science during a period of this length, so that the lectures will have a broad basis from which to draw their material.

VERY little doubt now exists as to the value of a captive balloon for reconnoitring purposes; and its importance has been lately displayed during the recent manœuvres. The military correspondent of the *Pall Mall Gazette* gives an interesting account of this special detachment; it consists of three officers and a dozen sappers. The balloon can lift 2½ cwt. The apparatus consists of a cart drawn by four horses, with two drums or winches, and round them is the hawser of twisted strands of wire which anchors the balloon. The balloon is generally allowed to ascend 1000 feet, and this is done by unwinding the wire. Messages are sent down in small bags fixed loosely on the hawser, and the officer at the bottom is either connected by wire with the General's headquarters, or has mounted orderlies at his disposal. A simple and rapid

means is adopted for bringing the balloon to earth. A stout pole, 10 feet long, is laid on the wire, and a couple of sappers take hold of each side, and double along, pressing on the pole all the time. The balloon is then made to come to earth in about two minutes; of course, some distance from the waggon. To let it up again, they double back, keeping the pole of the waggon horizontal; this system naturally saves much time and winding. To move the balloon from place to place, it is pulled down until it is on the top of the waggon, where it is held while the waggon is being driven along. The value of a balloon, the correspondent says, can hardly be over-estimated, both for reconnoitring the enemy and also as a means of keeping a General informed of the positions reached by his own troops. Sketches showing accurately all the enemy's dispositions can be thus easily obtained, which would otherwise have to be done by mounted troops, and, perhaps, not so thoroughly.

CONSIDERABLE danger appears to attend the consumption of the popular Norwegian cheese known as "Pult-ost" or "Knad-ost," in consequence of the vigorous kneading which it undergoes in process of manufacture. Several authentic instances of severe attacks of intestinal catarrh having followed the eating of this particular variety of cheese, attention has at last been directed to the subject, and an elaborate investigation has lately been carried out by Dr. Axel Holst, of Christiania. The bacteriology of the cheese in question has been specially studied, and elaborate examinations were made of those cheeses to which the above epidemic was attributed. The results obtained are of considerable interest; for not only has Dr. Holst isolated out the responsible microbe, but he has identified it as being a virulent variety of the *B. coli communis*. Thus fresh evidence is to hand in support of the now frequently expressed view that this microbe, so extensively present in our surroundings, and normally present in the animal system as a harmless saprophyte, may by some process, at present unknown to us, become endowed with highly toxic properties. Dr. Holst suggests that the original infection of the milk may have been brought about by want of cleanliness in milking, coupled with an unhealthy condition of the cow itself—circumstances which have already been proved to have induced diarrhoea in the case of persons who consumed such contaminated milk.

DR. C. HART MERRIAM, of the U.S. Department of Agriculture, has prepared a synopsis of the weasels of North America, which has just been issued from the Government printing office. Dr. Merriam recognises one ferret and twenty-two weasels, as being at present known in North America, and refers the former to the sub-genus *Putorius*, and the latter to the sub-genus *Ictis* of the same genus. Eleven species of North American weasels are now described as new for the first time. Five plates are added to this excellent memoir to illustrate the skulls and dentition of the various species.

FROM a memoir lately presented to the Imperial Academy of Sciences of St. Petersburg by Herr Eug. Büchner, it would appear that the European bison, in spite of the stringent efforts made by the Czars since the beginning of the present century for its protection, is likely to share the fate of its American relative, and to become extinct as a wild animal. The celebrated herd of the forest of Bjelowjesha, in Lithuania, which in 1856 was nearly 1900 in number, has of late years become reduced to under 500, and shows no signs of increase. The chief cause of this failing is attributed, by Herr Büchner, to "breeding in," in consequence of the confined area to which these huge animals are now restricted. Another reason is, no doubt, the fact that the male bisons considerably exceed the females in number. We venture to suggest that it would be wise to remove a large number of bisons of the male sex from the forest, and, if possible, to introduce a change of blood from

the second herd of the same animal which still exists on the northern slopes of the Caucasus.

A VERY interesting feature of primary education in Russia is the establishment and rapid development of small farms, orchards, and kitchen-gardens in connection with many primary schools, especially in the villages. The land for such model gardens, or farms on a small scale, was mostly obtained through free grants from the village communes and, occasionally, from the neighbouring landlords; while the expenses are covered by very small money grants from the country and district Councils (*zemstvos*). To take one province in South Russia, namely Ekaterinoslav, we see from the biennial report, just issued, that not only has almost every school an orchard and kitchen-garden for the use of the schoolmaster, but that nearly one-half of the schools in the province (227 out of 504) are already in possession of small model kitchen-gardens, orchards, tree-plantations, or farms, at which gardening, sylviculture, and sericulture are regularly taught. The teaching is mostly given by the schoolmasters, who themselves receive instruction in these respective branches at courses voluntarily attended in the summer, or occasionally by some practical specialist of the neighbourhood. The province of Ekaterinoslav being mostly treeless, special attention is given to tree plantations and, next, to silkworm culture. The aggregate area of the 227 school-farms or gardens attains 283 acres, and they contained, in 1895, 111,000 fruit trees and 238,300 planted forest trees; nearly 14,000 of the former and 42,000 of the latter having been distributed free among the pupils during the same year. The money grants for these 227 gardens were very small—i.e. a little over three hundred pounds (£314). Besides, over a thousand beehives are kept, partly by the schoolmasters and partly by the children; and some schools had vineyards in connection with them. This movement has widely spread over different provinces of Central Russia, where the culture of cereals dominates at the school farms; while in Caucasia attention is especially given to the silkworm culture and the culture of the vine.

THE following particulars of munificent gifts and bequests to libraries of America are gathered from *Science*:—The New York Free Library, from members of the Astor family, about £330,000; from James Lennox, £147,000, in addition to books and land; from the Tilden estate, £400,000; the John Crerar Library, of Chicago, from the founder, about £540,000; the Newberry Library, of Chicago, from the founder, about £500,000; the Carnegie Library, of Pittsburg, from the founder, £420,000; the Enoch Pratt Free Library, of Baltimore, from the founder, about £216,000; the Library Company, of Philadelphia, from the founder, Dr. Rush, about £212,000; the Library of Columbia University, from President Low, £200,000.

THE first article in the September part of the *Geological Magazine* is devoted to an historical account of the Palaeontographical Society of London, the jubilee of which was celebrated on June 19 last. The origin of the Society was mainly due to the prior issue of Sowerby's "Mineral Conchology," of which the first part appeared in June 1812, and was followed by other parts for over thirty years. The portions of this work were brought out slowly and irregularly, and rarely illustrated more than ten species at one time, and it was thought that as the "Mineral Conchology," at its then rate of issue, could not possibly depict all the British fossils within a moderate period, it would be well to have recourse to another method. The outcome of the suggestion made was the calling together of a meeting on March 23, 1847, at the apartments of the Geological Society, with Sir Henry De la Beche in the chair, when it was resolved that a Society be constituted, the object of which should be "to figure and describe as completely as possible a stratigraphical series of British fossils." The meeting

further determined that the annual subscription should be one guinea, that the name of the Society should be the Palæontographical, and that its officers should consist of a president, treasurer, secretary, and council of sixteen members. Sir Henry De la Beche was elected first president. The presidents from the foundation to the present have been: Sir H. De la Beche, from 1847 to 1855; Mr. W. J. Hamilton, from 1856 to 1867; Dr. Bowerbank, from 1868 to 1876; Sir R. Owen, from 1877 to 1892; Prof. Huxley, from 1893 to 1895; and since then Dr. Henry Woodward. Want of space will not permit of our giving a list of the monographs issued by the Society; suffice it to say that they range over a large area of information comprehending the fossil Plantæ, &c., fossils from the sub-kingdoms Protozoa, Porifera, Coelenterata, Echinodermata, Annulosa, Mollusca, and Vertebrata; and that monographs are in progress on the Foraminifera of the Crag, the Fossil Sponges, the Cretaceous Star-Fishes, the Carboniferous Mollusca, the Inferior Oolite Ammonites, the Fishes of the Old Red Sandstone, the Pleistocene Mammalia, and the Devonian Fauna. The Honorary Secretary of the Society, Prof. Wiltshire, will, we are sure, be glad to receive the names and subscriptions of many new members.

It may be remembered that some time ago the Austrian man-of-war *Pola* returned from her voyage of investigation in the Red Sea, after having spent eight months there, and surveyed the northern half of this region, covering an area of about 600 nautical miles long by 180 broad. An account of the work accomplished during this trip is given in *Die Natur* (No. 37), and seems to be of great importance. No less than seventy boxes, containing fish and smaller Seetiere, and twelve large boxes full of coral have already been despatched to Vienna. Other results of observation tell us that at a depth of 500 metres the water commences to become homogeneously warm, having a temperature to the bottom of $21^{\circ}2$. The amount of salt at the time of observation was greatest in the northern part, diminishing towards the south; the transparency of the water was found to be less than that of the Mediterranean Sea; and the colour was not so fine and of such a distinct blue as is the case of the Mediterranean and Adriatic Seas. Altogether 1243 temperature observations, 691 determinations of the specific gravity at the surface, half-way down, and at the bottom, 254 colour observations, and 22 determinations of wave-elements were made. The investigation included further innumerable chemical analyses of the water from the deeper parts, and observations of the chemical changes taking place between the Red Sea and the land surfaces. At most of the harbours and places of anchorage the officers made astronomical position and time determinations, magnetic observations for all three elements—declination, inclination, and intensity, and pendulum observations for the determination of the force of gravity. The above were made at twenty-seven stations on land and islands, twelve being on the Egyptian coast, ten on the Arabian, and the rest on the half-island Sinai. Meteorological observations were also strictly made, thus completing a valuable amount of work in an important region.

THE current number of *La Nature* (September 12) has some specially interesting articles. M. de Navailles gives us an account of some of the prehistoric finds that have been made in Florida, referring to the work done especially by Mr. Frank Cushing, who was sent out by the University of Pennsylvania. His researches seem to have been very successful, and he was rewarded with sufficient material to permit him to describe the life and costumes of the race that had peopled the land. Dr. Felix Regnault discourses on the origin of ornamental art: the principles which actually guide ornamental art date neither from the Renaissance, nor even from the Greek period. They result

“d’une tendance naturelle à l’homme telle qu’on retrouve dans les origines mêmes de l’humanité.” “Renaissance Clocks” is the subject of an article by M. Planchon, who describes some of these beautiful works of art. There are several illustrations showing the different methods of design adopted.

THE *Bulletin de la Société d’Encouragement pour l’industrie Nationale* for the month of August contains various articles which should be read with interest. M. Ronna, in a long summary of sixty-four pages, gives an account of the dry regions in the United States of America, and describes at some length the different methods of irrigation employed to suit the various local conditions. Plans, cross-sections and photogravures are given to illustrate the regions under discussion, and these give us a good idea of the enormous scale on which some of the undertakings are made. The first part deals chiefly with the distribution of water, by the formation of reservoirs, and the subsequent building of canals. The question of wind and steam pumps then passes under review; here all kinds of types are mentioned, including an account of some of the more important artesian wells. Then follows the different systems adopted for actually distributing the water over areas, such as, for instance, a plantation; these vary according to the amount of water required. The statistics brought together show the enormous increase in the productive value of the land since the adoption of irrigation on a large scale. Under the heading of “Metallurgy,” Roberts-Austen’s and F. Osmond’s researches on the structure of metals are translated.

IN the current number of the *Annales de Chim. et de Phys.*, M. Moissan gives an interesting account of his experiments on the volatilisation of refractory substances in the electric furnace. The sublimes were condensed on the outside of a curved copper tube placed two centimetres below the arc, and just above the substance under examination. A rapid current of water was passed through the tube, and kept it cool during the experiments, which usually lasted for about five minutes. The volatilised metals were copper, silver, platinum, aluminium, tin, gold, manganese, iron, and uranium. Quantitative experiments were not made in every case, but it appeared that manganese was sublimed more rapidly than the others, and that the rate of volatilisation of copper was about five times as rapid as that of gold. The condensed metal was usually, in great part, in the form of little spheres. Silicon and carbon were also volatilised and condensed on the tube, though the amount collected of the last-named element was very small, and lime, magnesia, zirconia and silica were sublimed without difficulty. M. Moissan draws the conclusion that the most stable compounds hitherto known disappear in the electric furnace, being either decomposed or volatilised. Nothing resists these high temperatures except the series of perfectly crystallised compounds, discovered by him, and consisting of borides, silicides, and, above all, carbides of the metals. M. Moissan intends to publish a description of these compounds shortly. He regards them as being probably among the original constituents of the globe, and as still existing in some of the stars.

THE earliest recorded measurements of the “dip” of the earth’s magnetism were made in London by Robert Norman in the year 1576, and by Gilbert in 1600; but it was not until 1671 that this element formed the subject of a series of regular observations at Paris, indicating a continual diminution during the last two centuries from 75° to $65^{\circ}5'$. In endeavouring to trace the secular variation of the dip, it is important to obtain, if possible, data extending over a far longer period. In a highly suggestive paper published in the *Atti dei Lincei*, Dr. G. Folgheraiter, taking as his starting-point the well-known property possessed by clay after it has been baked of retaining permanently any magnetisation that may have been induced in

it during the process of baking, advances the view that the various objects of terra-cotta discovered in excavations (vases, bricks, &c.) afford an indelible record of the state of the earth's magnetism at the epoch of their fabrication. The author, besides citing the observations of a number of physicists in support of his view, has put the matter to a practical test by examining the magnetisation of the bricks used in the construction of ancient villas, tombs, &c., at the time of the Roman Empire, of which the remains abound in Rome and the Campagna. It was found that the direction of magnetisation varied from brick to brick, some in the course of building having been laid with their axes of magnetisation in the opposite direction to the earth's magnetic force, some with their axes in the same direction, while others were magnetised normally to that direction; in no case did the axis of magnetisation correspond to any fixed direction, thus proving that the bricks had retained their polarity unaffected by terrestrial magnetism during the many centuries that have elapsed since the buildings were constructed. An examination of some Etruscan vases, dating from the eighth century B.C., leads to similar conclusions. In a tomb recently discovered at Narce (now Calcata), one of two vases was found magnetised in a nearly horizontal direction, the other exhibited a decided south pole at the bottom, and a north pole at the top, while two large *cratere* had their north poles in the centre of the base, and their south poles in the orifice at the top. In a tomb at Falerii, two vases were found having the south pole in the centre of the base, while one "oinochōē" (wine measure), accompanying them, had the north pole at the upper extremity of its beak. From such evidence Dr. Folgheraiter considers himself justified in asserting with certainty that the direction of magnetisation observed in antique objects of terra-cotta is that due to the inducing effect of the earth's magnetism during the process of baking. In this, his first paper, the author draws no inferences relating to the earth's magnetism, but he points out a difficulty arises in most instances, owing to our uncertainty as to the orientation of the objects when originally placed in the kiln, a factor which must evidently be known before any definite conclusions can be stated.

THE August number of the *Proceedings of the Geologists' Association* contains a paper by Dr. Hicks on the Palæozoic Rocks of West Somerset and North Devon, in which are some original illustrations of the remarkable folds in the rocks of the Ilfracombe district. This is followed by a preliminary Synopsis of the Fauna of the Pickwell Down, Baggy, and Pilton Beds, by the Rev. G. F. Whidborne, in which brief descriptions of some seventy-four new species are included. The Rev. H. H. Winwood contributes notes on the Trias, Rhatic and Lias of West Somerset. These three papers have already been issued as the usual Long Excursion pamphlet. In addition the number contains a paper by Mr. A. E. Salter on the "Pebbly Gravel" of South-east England, and the relation of its distribution to the gaps in the present line of high-ground between Goring and the Norfolk coast; and one, by Mr. Strahan, on the physical geology of Purbeck and the relations of the anticlines to the watersheds in the south and south-east of England.

THE recent number of the *Proceedings of the Liverpool Geological Society* (vol. vii., part iv., dated 1896 only) contains an important presidential address by Mr. Mellard Reade, on British Geology in relation to Earth-Folding and Faulting; while a contribution to the same subject, in respect of the Craven district, is made by the Rev. F. F. Grensted. Among other papers of more than local interest we may mention Mr. Beasley's Attempt to classify the Footprints from the Trias of the district; Mr. Mellard Reade's Notes on the Drift of Mid-Wales; Mr. Lomas's Observations on Recent Glacial Striae in Switzerland; and Dr. Callaway's Sketch of the Process of Metamorphism in the Malvern Crystallines.

NO. 1403, VOL. 54]

Science for September 4 contains a lengthy appreciative article on Dr. P. L. Sclater, F.R.S., from the pen of Dr. G. Brown Goode.

THE Annual Report of the Keeper of the Manchester Museum has reached us, and is of a very encouraging character. During the year the Museum was recognised in a practical manner by the City Council as a public institution, for on October 16, 1895, a resolution was passed at a meeting of the Council that the sum of £400 a year should be granted to the Manchester Museum from the Free Library Rate. In November the experiment of opening the Museum for two hours on Sunday afternoons was first made. On the opening day, November 17, 494 persons attended. Since that date the attendance has varied from 131 to 797, the average being 519. During the year the Museum has received a large number of gifts from individuals and institutions.

VOL. XXVIII. of the *Transactions and Proceedings of the New Zealand Institute* (published in this country by Messrs. Kegan Paul and Co., Ltd.) is a bulky volume of 787 pages, and contains seventy-four articles classified as miscellaneous, or relating to zoology, botany, geology, and chemistry. In addition to these, reports of the proceedings of the incorporated societies are given, and the whole is enriched by thirty-seven well-executed plates.

THE Annual Report of the Calcutta Botanic Gardens, issued by the Superintendent, Dr. G. King, shows steady work and increase in efficiency in the various departments, notwithstanding the severe injury inflicted by the unusual drought of the summer months (October to March). The work of the herbarium has been carried on with vigour, a very large number of specimens have been added to it, while named specimens of Indian plants have been forwarded to various scientific institutions throughout the world.

THE *Botanical Gazette* records the establishment of a Biological Survey by the Department of Agriculture, under a recent Act of Congress of the United States, which it regards as the beginning of a new era in the botanical field-work of the States. The head of the new Survey will be Dr. Merriam, who has had great experience in the kind of work which it proposes to undertake.

THE corner-stones of the Hull Biological Laboratory of the University of Chicago were laid in July last. The Botanical Hall is expected to be finished by the spring of 1897, and, if we may judge from a sketch in the *Botanical Gazette*, will be a very imposing structure.

VOL. VI. part ii. of "Flora Capensis: being a Systematic Description of the Plants of the Cape Colony, Caffraria, and Port Natal (and neighbouring territories)," by various botanists, and edited by Mr. W. T. Thiselton-Dyer, is almost ready for publication by Messrs. L. Reeve and Co. Like part i. of vol. vi., the present contribution is the work of Mr. J. G. Baker, the keeper of the Herbarium and Library of the Royal Gardens, Kew, and contains the continuation of the *Amaryllideæ* and part of the *Liliaceæ*, to the completion of which the whole of the third and concluding part will be devoted.

MESSRS. RIVINGTON, PERCIVAL, AND Co. will publish shortly, "Mechanics for Beginners treated Experimentally, embracing Statics, Dynamics and Hydrostatics," by L. Cumming, of Rugby School.

MESSRS. HENRY HOLT AND Co., New York, hope to issue at an early date a translation, by Prof. G. W. Field, of Brown University, of the first or "general" part of Dr. Hertwig's "Lehrbuch der Zoologie."

A NEW edition of Dr. Oliver Lodge's "Elementary Mechanics, including Hydrostatics and Pneumatics" has just reached us from Messrs. W. and R. Chambers, Ltd. It has been completely revised by the author and by Prof. Alfred Lodge.

MESSRS. TAYLOR BROTHERS, Leeds, have sent us the second edition of "The Collector's Manual of British Land and Freshwater Shells," by L. E. Adams. The preface states that the book has been re-written and brought up to date.

THE additions to the Zoological Society's Gardens during the past week include two Bonnet Monkeys (*Macacus sinicus*, ♂ ♀) from India, presented respectively by Mr. John Hart and Mr. E. E. Hodgskins; a Rhesus Monkey (*Macacus rhesus*, ♀) from India, presented by Mr. Frederick Tomlin; a Mozambique (*Cercopithecus pygerythrus*, ♂) from South-east Africa, presented by Mr. A. C. Jackson; two Lanner Falcons (*Falco lanarius*), South European, presented by Mr. W. Glynes Bruty; a Glaucous Gull (*Larus glaucus*) from Franz Josef Land, presented by the Jackson-Harmsworth Polar Expedition; a Raven (*Corvus corax*), British, presented by Mr. O. L. Pegler; an Egyptian Jerboa (*Dipus agyptius*) from North Africa, a Rat-tailed Serpent (*Trigonoccephalus lanceolatus*) from St. Lucia, W.I., deposited; a Diana Monkey (*Cercopithecus diana*, ♀) from West Africa, three Capceira Partridges (*Odontophorus dentatus*) from Brazil, purchased; a Red Deer (*Cervus elaphus*, ♀) from Scotland, received in exchange; two Triangular-spotted Pigeons (*Columba guinea*), a White-backed Pigeon (*Columba leuconota*), two Half-collared Doves (*Turtur semitorquatus*) bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE RECENT SOLAR ECLIPSE.—M. Tisserand furnishes a few details on the results obtained by the Russian astronomers during the eclipse of August last, which were communicated to him through M. Backlund, Director of the Observatory of Pulkowa. M. Backlund's station was situated in Novaya Zemlya, where he landed three weeks before the day of the eclipse. During this period the sky remained constantly cloudy, the temperature varying from 0° to 3°. During some occasionally bright moments altitudes of the sun were obtained to check their chronometers and determine their rates. M. Galitzine made a series of magnetic observations. At four o'clock on the morning of the eclipse the sky was still overcast, but the weather cleared up somewhat, and the observers were able to observe the four contacts under good conditions. Clouds, however, were not entirely absent; but successful observations and photographs of the corona were taken.

M. Eugène-M. Antoniadi, in the September number of the *Bulletin de la Société Astronomique de France*, gives an account of his trip in the *Norse King* to Vadsö. The article in question is illustrated, and contains, among others, a drawing of the region in and about Vadsö, and an excellent photograph of the town of Hammerfest.

COMET BROOKS.—A Centralstelle Circular, dated September 10, gives an ephemeris, with elements, of this comet computed by Prof. E. Lamp from observations made on September 7, 8, and 9. They are as follows:—

$$\begin{aligned} \text{Elements.} \\ T &= 1896 \text{ July } 7^{\text{h}} 27^{\text{m}} 8^{\text{s}} \text{ Berlin M.T.} \\ \omega &= 38^{\circ} 22' \\ \Omega &= 149^{\circ} 22' \\ i &= 88^{\circ} 16' \\ \log q &= 0.06497. \end{aligned} \quad 1896^{\circ}$$

Ephemeris 12h. Berlin M.T.

1896.	h.	m.	δ	log Δ	B.
Sept. 16	14	55.9	+53 15	0.2405	0.9
20	15	21.0	51 53	0.2428	0.8
24	15	44.7	50 16	0.2463	0.8
28	16	6.9	48 26	0.2512	0.7
Oct. 2	16	27.6	46 29	0.2574	0.7
6	16	46.7	44 25	0.2650	0.6

The unit of brightness occurred on September 4.

NO. 1403, VOL. 54]

COMET GIACOBINI.—A circular from Kiel, dated September 9, gives the elements and ephemeris of this comet calculated by Dr. H. Kreutz from observations on September 5, 6, and 7.

$$\begin{aligned} \text{Elements.} \\ T &= 1896 \text{ October } 8^{\text{h}} 00^{\text{m}} 3^{\text{s}} \text{ Berlin M.T.} \\ \omega &= 155^{\circ} 2' 1'' \\ \Omega &= 195^{\circ} 39' 5'' \\ i &= 8^{\circ} 45' 2'' \\ \log q &= 0.04004 \end{aligned} \quad 1896^{\circ}$$

These elements are stated to be somewhat uncertain. The orbit is probably an ellipse.

Ephemeris 12h. Berlin M.T.

1896.	h.	m.	δ	log Δ	B.
Sept. 7	17	16.5	-8 0	9.785	1.1
11	17	25.1	-8 41	9.772	1.2
15	17	35.0	-9 24	9.757	1.3
19	17	46.4	-10 10	9.742	1.4
23	17	59.3	-10 58	9.7050	1.6
27	18	13.9	-11 47	9.709	1.8

NEW FEATURE ON MARS.—With reference to the recent observation of bright light on the terminator of Mars, referred to on September 3, we may state that the bright prominences (mountains?) on the terminator of Mars were first seen by the astronomers of the Lick Observatory in 1890, and have been regularly observed (measured) at subsequent oppositions by the observers at Mount Hamilton, Nice, and Flagstaff.

Prof. Hussey observed their first appearance in 1896 on August 28, oh. 45m. Greenwich mean time, in the *Chersonesus* region of Mars.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

It is announced that a sum of between £15,000 and £17,000 is to be spent in providing buildings and fixtures for the Sunderland Technical College.

AMONG recent appointments we notice the following:—Mr. Lake becomes Principal of the Technical and University Extension College, Colchester; and Mr. J. W. B. Adams, Head-master of the Tenby and Intermediate and Technical Schools.

MR. A. M. DRENNAN, of the Burnley Grammar School, has been appointed Head-master of the Banbury Municipal Secondary and Technical School.

AMONG recent appointments abroad may be mentioned:—Prof. W. Dames to succeed the late Prof. Beyrich in the chair of Geology and Paleontology at Berlin; and Dr. W. Wien, to be Associate Professor of Physics at Berlin.

THE Chemical Laboratory of the University of Illinois has been destroyed by fire. It is reported to have been one of the largest and best-appointed of its kind in the country, and was erected at a cost of about £8000. Its fittings, apparatus, &c., are said to have added to this amount about £7000.

THE Syllabus of the Municipal Technical School and Municipal School of Art, Manchester, for the forthcoming session is now ready. It is issued by Mr. John Heywood, Manchester. The Calendar for the twelfth session of the Merchant Venturers' Technical College, Bristol, is also ready, and may be had, at a small charge, of Mr. H. Y. Hill, Bristol. We have also had sent to us the prospectus of day and evening classes to be conducted at the Battersea Polytechnic Institute. Copies may be obtained on application to the Secretary.

THE fifth annual report of the Department of Agriculture of the Yorkshire College, Leeds, is a record betokening much activity. Most of the lecture courses appear to have been well attended, though in the teachers' agricultural classes the attendance fell off so considerably that it was found impossible to hold a summer vacation course. In the winter courses the same difficulty was experienced, and though attempts were made to form classes at five centres, it was only possible to obtain sufficient students to form one class. At the request of the West Riding County Council, the department formulated a scheme for the institution of gardens for instruction in horticulture in connection with evening continuation schools. The scheme has been adopted by the West Riding County Council, and grants have been promised by them provided certain regulations are complied with.

SCIENTIFIC SERIAL.

American Journal of Science, September.—On the regular or specular reflection of the Röntgen rays from polished metallic surfaces, by O. N. Rood. Platinum foil at an angle of incidence of 45° reflects $\frac{1}{17}$ th part of the incident X-rays. About half the rays are reflected in a regular geometrical or specular manner, as proved by photographs of iron gratings obtained by means of the reflected rays, and compared with photographs obtained with the same mirrors by means of ordinary light, diffused or radiating from a point. But the proportion of regularly reflected rays is less than in the case of ordinary light. There is a greater proportion of diffused rays, but these are diffused, not as they would be by a dull surface, but as they would be by an imperfectly polished surface. Similar results were obtained with speculum metal and tinfoil.—An iodometric method for the determination of phosphorus in iron, by Charlotte Fairbanks. Phosphorus may be determined in iron by precipitating the ammonium phospho-molybdate according to the usual methods of iron analysis; then reducing the phospho-molybdate thus obtained with potassium iodide and hydrochloric acid; neutralising the residue with acid sodium carbonate, and reoxidising with standard iodine.—Is the land round Hudson Bay at present rising? by J. B. Tyrrell. The reasons advanced by Dr. Robert Bell for supposing that the land round Hudson Bay is still rising are not conclusive. The land at the mouth of the Churchill River has been unchanged for the last century and a half. Sloop Cove, where the sloops engaged on the Eskimo trade used to winter, has many inscriptions of the middle of the eighteenth century, whose position, when compared with their exact date, shows that they would not have been hewn into the rock at the level they occupy if the tides had at that time attained a higher level than they do now.—A visit to the Great Barrier Reef of Australia, by A. Agassiz. The expedition, supported by the United States, the British, and the Queensland Governments, was equipped for extensive pelagic fishing and topographical surveying inside and outside the Barrier Reef. Boisterous weather made pelagic fishing very difficult, and the explorers had to content themselves with an examination of the inner portions. The slope is greatest in the southern portion, where the channel is wider. There is evidence to show that the islands composing the reef formerly filled up the channel as well. The islands lining the continent were the last to disappear. The very moderate subsidence which has taken place in comparatively recent times cannot have shaped the outlines of the present Australian continent, and of its submarine extension. For this we must look back, first to the subsidence which took place in Cretaceous times, next to the subsequent elevation of the Cretaceous beds, and finally to the erosion and denudation to which these beds, since their elevation above the level of the sea, have for so long a period been subjected. It is on the upper part of these submarine slopes, dating back to an earlier geological period, but modified by erosion and denudation up to recent times, that during the present epoch corals have obtained a footing and built up the Great Barrier Reef of Australia.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, September 7.—M. A. Cornu in the chair.—Notice on the late Amé-Henry Resal, by M. Maurice Levy.—On the observations of the eclipse of the sun of August 9 last. Extract from a letter to M. Tisserand by M. Backlund, Director of the Observatory of Pulkowa. Although for some time preceding the eclipse the weather had been extremely unfavourable, the first contact was observed in a perfectly clear sky, and a dozen good photographs were obtained.—Memoir on the thermo-chemistry of the oxygen compounds of phosphorus, arsenic and sulphur, by M. Marcellin Langlois.—On the steering of aerostats, by M. Rozier.—On the employment of a fixed circle, derived from any group of seven tangents to a conic, to define, *à priori*, the circle derived from any seven right lines, by M. Paul Serret.—On the distribution of deformations in metals submitted to stresses, by M. L. Hartmann. A reply to some remarks by M. Charpy.—Discharge of electrified bodies by the X-rays, by M. E. Villari. The experiments described tend to show that the discharge of electrified bodies is not brought about by the X-rays themselves, but by the air rendered

active by their passage.—On the emission of the X-rays, by M. C. E. Guillaume. A theoretical proof of the laws of emission established experimentally by MM. Imbert and Bertin-Sans, and by M. Gouy.—On the general relation between the intensity of sensation and the duration of a luminous impression, by M. Charles Henry.—On some questions in celestial mechanics, by M. A. Karagiannides.—On nervo-psychosis, by M. Bouxteiff.

SYDNEY.

Royal Society of New South Wales, June 3.—Mr. J. H. Maiden, President, in the chair.—The following papers were read:—On periodicity of good and bad seasons, by Mr. H. C. Russell, C.M.G., F.R.S.—The Mika operation of the Australian Aborigines, by Prof. Anderson Stuart.—The absorption of water by the gluten of different wheats, by Mr. F. B. Guthrie.

July 1.—Mr. J. H. Maiden, President, in the chair.—Discussion upon the paper read by Mr. Russell at the preceding meeting.—Notes on recent developments of Röntgen rays, by Prof. Threlfall.

August 5.—Mr. J. H. Maiden, President, in the chair.—Papers read:—On the occurrence of a submerged forest with remains of the dugong at Shea's Creek, by Mr. R. Etheridge, jun., Prof. T. W. E. David, and Mr. J. W. Grimshaw (with exhibits).—On aromadendrin or aromadendric acid from the turbid group of *Eucalyptus kinos*, by Mr. Henry G. Smith.—On the cellular kite, by Mr. Lawrence Hargrave (with exhibit).—Note on a method of separating colloids from crystalloids by filtration (with demonstration); also an explanation of the marked difference in the effects produced by subcutaneous and intravenous injection of the venom of Australian snakes, by Dr. C. J. Martin.—Mr. H. G. Smith exhibited a specimen of Lapidolite (*Lithium mica*) from near Norseman, West Australia.

CONTENTS.

PAGE

Lydekker's Geographical History of Mammals . . .	457
The Rational Study of Plant-Distribution. By J. B. F.	458
Our Book Shelf:—	
Vernon-Harcourt: "Rivers and Canals"	459
Cloves and Coleman: "Elementary Practical Chemistry and Qualitative Analysis"	460
Morley: "Entomological Notes for the Young Collector."—W. F. K.	460
Letters to the Editor:—	
The Utility of Specific Characters.—Prof. Karl Pearson, F.R.S.	460
Specific Characters among the Mutillidæ.—Prof. T. D. A. Cockerell	461
The Khmer of Kamboja.—S. E. Peal	461
Dr. Siemens' Smokeless Open Grate.—Fred. Wm. Foster	462
The Liverpool Meeting of the British Association. IV. By Prof. W. A. Herdman, F.R.S.	462
Inaugural Address by Sir Joseph Lister, Bart., P.R.S., President	463
Section A—Mathematics and Physics.—Opening Address by Prof. J. J. Thomson, F.R.S., President of the Section	471
Section B—Chemistry.—Opening Address by Dr. Ludwig Mond, F.R.S., President of the Section	475
The American Association for the Advancement of Science	480
The Recent Cyclone in Paris	481
Prehistoric European Art	482
Notes	482
Our Astronomical Column:—	
The Recent Solar Eclipse	487
Comet Brooks	487
Comet Giacobini	487
New Feature on Mars	487
University and Educational Intelligence	487
Scientific Serial	488
Societies and Academies	488

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H.
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ralia.

PAGE

. 457

y
458

. 459

al
. 460

g
. 460

rl
. 460

t.
. 461

n.
. 461

n.
. 462

n.
. 462

t.,
. 463

ng
S.,
. 471

Dr.
on 475

of
. 480

. 481

. 482

. 482

. 487

. 487

. 487

. 487

. 488

. 488